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Journal of Experimental Psychology

EDITED BY

MADISON BENTLEY, CORNELL UNIVERSITY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
RAYMOND DODGE, YALE UNIVERSITY (*Monographs*)
S. W. FERNBERGER, UNIVERSITY OF PENNSYLVANIA (*Bulletin*)
WALTER S. HUNTER, CLARK UNIVERSITY (*Index*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY, Business Editor

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CONFIGURATION AND BRIGHTNESS CONTRAST

BY W. H. MIKESELL AND MADISON BENTLEY[1]

It is a commonplace in psychology that a local gray area set in a general visual field depends for its phenomenal quality both upon the character of the local light stimulus and upon the filling of the general field. The most obvious demonstration of this dependence is made by placing two like gray papers right and left, the one upon white paper and the other upon black. The gray-on-white looks darker than the gray-on-black. This phenomenal difference has commonly been regarded as brightness contrast, to be explained by a mutual excitatory influence of neighboring retinal areas. But champions of the *Gestalt*,[2] using certain more complicated visual patterns, have recently contended that visual contrast does not adequately account for the observed change in local grays when the grays are inserted in geometrical designs. Wertheimer cut out a heavy black cross and exposed it upon a white background. Two small triangles of like size and shape were then cut from the same piece of gray paper. One

[1] The experiments were carried out at the University of Illinois.

[2] W. Benary, Beobachtungen zu einem Experiment über Helligkeitskontrast, *Psychol. Forsch.*, 1924, 5, 131–142; M. Eberhardt, Untersuchungen über Farbschwellen und Farbenkontrast, *ibid.*, 1924, 5, 85–130; W. Fuchs, Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten, *Zsch. f. Psychol.*, 1923, 92, 298 ff; A. Gelb, Über den Wegfall von 'Oberflächen Farben,' *ibid.*, 1920, 84, 193; A. Gelb & R. Granit, Die Bedeutung von 'Figur' und 'Grund' für die Farbenschwelle, *ibid.*, 1923, 93, 83; E. Rubin, Visuell wahrgenommene Figuren, Berlin, 1921; M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 1921, 1, 47–58; 1923, 4, 301–350; 1925, 7, 81–136; K. Koffka, Über Feldbegrenzung und Felderfüllung, *ibid.*, 1923, 4, 176–203; W. Köhler, Die physischen Gestalten in Ruhe und im stationären Zustand, 1920; H. Werner, Studien über Strukturgesetze, *Zsch. f. Psychol.*, 1924, 94, 252 ff.

triangle was placed upon a limb of the black cross and the other just outside the cross but within an angle formed by two adjacent limbs. Contrast, which moves in the direction of 'opposition,' would lead one to expect the gray which is adjacent to most white to be darker and the gray which is adjacent to most black to be lighter; but Wertheimer saw exactly the opposite. The gray on the black design (G_i, inside gray) looked lighter than the outside gray (G_o), although more white actually surrounded it. Adherents of the doctrine of *Gestalt* have interpreted this effect as due to the fact that G_i was in the *Gestalt* or *configuration* while G_o was outside. G_i was a part of the 'figure' and G_o was not. G_i belonged-to or inhered-in the figure while G_o did not. It was for some the figural inherence (*Zugehörigkeit*) which determined the quality of the first gray. This was in line with the contention of the *Gestalttheorie* that the whole determines the parts. Benary, who repeated Wertheimer's observation, also used a cross-and-triangle figure, a thick 'H' and 'I' and a heavy C-figure. The general outcome was the same in all, and it was to some extent confirmed when colored patches replaced the 'critical' grays. Since both procedure and interpretation seemed open to criticism, we have repeated and extended Benary's experiments.

Our criticism of method runs as follows.

(1) Benary's instruction to the obervers was not controlled. It seems to have been a casual request to look at the figure and to report.

(2) The general conditions of exposure were inadequately controlled. Illumination is not specified. O sat at various distances (0.5 to 14 meters). Exposure was of variable duration. Shift of fixation, eye-movement, after-images, adaptation and induction were not strictly provided against. Conditions during rest-intervals were not sufficiently taken into account. The number of observations for each design is not given for the several Os.

(3) B seems to identify his geometrical designs with his psychological configurations. That the O will 'configure' the design presented and will form a 'ground' of the background is a sheer assumption which frequently recurs in the study of visual perceptions made by the adherents of this doctrine. The fact of configuration should always be attested by O's report and not inferred from geometry. *Design* and *seen figure* (the alleged *Gestalt*) are constantly confused by B.

(4) As a result of this confusion, 'inherence' becomes a doubtful concept. Frequently it seems to mean no more than spatial inclusion in the design. That there was a *psychological* inherence is not sufficiently attested and just what it would be, in the sense of the *Gestalt*, is not shown. In some of our own experiments, it

appeared that the spatially included gray might well be an *intrusion* rather than a configured member, and so *resist* membership.

Preliminary observations. We repeated B's observations under the following conditions. The designs were copied as well as they could be from B's defective descriptions. They were pasted on large black or white sheets of cardboard (22 in. x 28 in.). Holes of the size and form of the critical grays were cut in the black or white designs. Behind them were placed homogeneous sheets of the critical gray. The whole was evenly illuminated by a 400-watt artificial (Corning) daylight bulb. This exposure field was covered by black when O entered the room. O was seated behind a hooded screen, with a head-rest, and the exposure was governed by raising a shutter before his eyes. The cards were exposed 1 to 15 sec, after which O was asked to report.

The instruction was as follows:

"At the signal 'ready' fixate. At the signal 'now' compare in quality the patches of gray in the total field. Be prepared at the signal 'now' to observe plane designs of black, white and gray. Report your comparison in one of the four degrees of assurance: high (4), moderate (3), small (2), and wanting (1). Do not fixate any point of an object and be prepared for a brief exposure."

The preliminary trials [3] generally confirm Benary's result. This method of Benary is, however, plainly defective. We have every reason to believe that the gray qualities compared were determined in part by the variable excitations of the retina as the designs were examined. Two photometrically equated lights would naturally be reported as 'different' when foveal vision passed to one of them from a black surface and to the other from a white surface. It is obvious that such a research as ours must control the state of the entire retina throughout the entire period of excitation.

To this end a camera shutter was secured with a calibrated exposure-range of 3 σ to 1000 σ. We used an exposure time of approx. 25 σ, in order to fall below the limits for eye-movement. The shutter was placed in the front wall of a light-box before the 400-watt lamp, with a water-cell to prevent damage to the shutter by heat. The light was reduced by ground-glass plates to eliminate after-images. Except for the

[3] The observers for these first trials were Drs. C. R. Griffith and G. D. Higginson, instructors in the Department, Misses R. Franklin and E. Rutherford, graduate students.

flash upon the exposure field, the room was dark. A faint fixation-light was arranged near the middle of the field and O held this in steady foveal vision just before the flash revealed the objects in the field. The field was 3 meters distant from the eyes. As the critical grays were to be differently oriented in the various patterns and objects to be presented, O had to be prepared each time for the approximate placing of the impending grays. This was arranged by a brief pre-exposure (in the direction of the field) of an outline sketch of the two critical grays oriented as they were about to appear. This sketch was weakly illumined in a frame set before the front opening of the hood and was removed just before the 'ready' for the main exposure of the field.

Five minutes were allowed for partial dark-adaptation. Observations fell within an hour, care being taken to prevent fatigue and to provide sufficient intervals between exposures.

The Main Experiments

Cards were made by pasting designs, together with the critical grays, on various backgrounds. The critical grays were observed to be phenomenally equal before being pasted in place. Each card was presented in four positions; straight up, inverted, and on its right and left.

The instructions follow.

"Before the main exposure-field is presented, you will be shown a sketch to indicate the relative positions of the fixation point and two gray areas. Simply note these relations.

After the sketch is removed, take a comfortable observing position at the caution 'prepare.' When the fixation light appears, *hold* it. After a short interval will appear the two gray areas in the places indicated by the sketch. Compare the grays with respect to *quality* only.

Report 'same' or 'right darker' ('lighter') or 'left lighter' ('darker') or 'upper darker' ('lighter') or 'lower darker' ('lighter'). If a report is not formulated, simply say 'no report.'

Always add to your report the degree of assurance.

4 means high assurance (virtual certainty)
3 " moderate assurance (moderate certainty)
2 " small assurance (moderate uncertainty)
1 " lack of assurance (virtual uncertainty)

Note well the following cautions:

(1) Make sure of steady fixation. Comment upon any slip. Be prepared to answer inquiries upon steadiness during the appearance of the fixation-light.
(2) The exposures will be brief; be prepared.
(3) Remember that 'no report,' 'small assurance' or 'same' has the same psychological value as reports of clear difference and of high certainty.
(4) Be on your guard against self-instruction, bias, and reflective judgments. Report photographically and promptly.
(5) Do not complicate your task, which is *only* to observe the grays with respect to likeness or difference of quality.
(6) Avoid discussion of this problem with the experimenter."

Series I: Benary Designs and Variants of Them. Eighteen patterns (numbered 1–18 below) of the Benary type were used. Twelve of them were repetitions of Benary's actual designs with variation in size, brightness, design and background. They are described as follows.

(*a*) Four variations of the cross design in *black upon white* background (dimensions in cm).

	Horiz.	Vert.	Crit. Grays (Qual.)	Crit. Grays (Size)
(1) Cross	6	8	Medium	1.5 x 3.6 x 4.0
(2) "	6	8	Medium	4.0 x 8.0 x 10.0
(3) "	6	8	Dark	4.0 x 7.0 x 7.0
(4) "	3	4	Medium	0.8 x 1.8 x 2.0

(*b*) Two variations of the cross design in *white upon black* background.

	Horiz.	Vert.	Crit. Grays	Crit. Grays
(5) Cross	6	8	Medium	1.5 x 3.6 x 4.0
(6) "	3	4	Dark	0.8 x 1.8 x 2.0

(*c*) One dark gray cross upon white background with medium gray critical fields.
(7) Same size as (6)[4]

(*d*) Two triangle-cross patterns: one *black on white* (See Fig. 1, Ser. I–8), the other *white on black*.

	Triangle	Cross		
		Horiz.	Vert.	Crit. Grays
(8, 9)	6 x 7.9 x 7.7	3	4	0.8 x 1.8 x 2.0

(*e*) Two H-I patterns: one *black on white*, the other *white on black* (Fig. 1, Ser. I–10).

	The "H"		The "I"	
	Vert. Beams	Crossbeams	Crit. Grays (rect.)	
(10, 11)	3 x 10	3 x 3	3 x 10	2.0 x 3.0

(*f*) Two equally weighted patterns which are so made up that a left black design is mortised on the right side and into this opening fits a corresponding tenon of a white design. The patterns balance each other. G_i (inside gray) is placed in the black and G_o (outside gray) in the white design.
(12)

(*g*) The first departure from Benary's design, but preserving the type (Fig. 1, Ser. I–13). Two large broad "L's": 13, *black on white;* 14, *white on black.* The critical fields were so arranged that G_i and G_o had equal areas of adjacent black. We sought here and in the succeeding patterns to devise less complicated objects than Benary's.

	Length	Width	Width of Horiz.	Crit. Grays
(13, 14)	12	12	6	3 x 4 x 5

(*h*) Two I–L designs: one *black on white*, the other *white on black*. The critical field in the "I" was adjacent to very little black or white in comparison with the G_o gray. The letters were more widely separated than the cut (Fig. 1, Ser. I–15) shows.

	"I"	Length of "L"	Width of "L"	Crit. Grays
(15, 16)	2.8 x 9	11	4 (top) 6 (bottom)	2 x 3 8 x 4.2

[4] All the critical fields of the designs following No. 6 were medium gray.

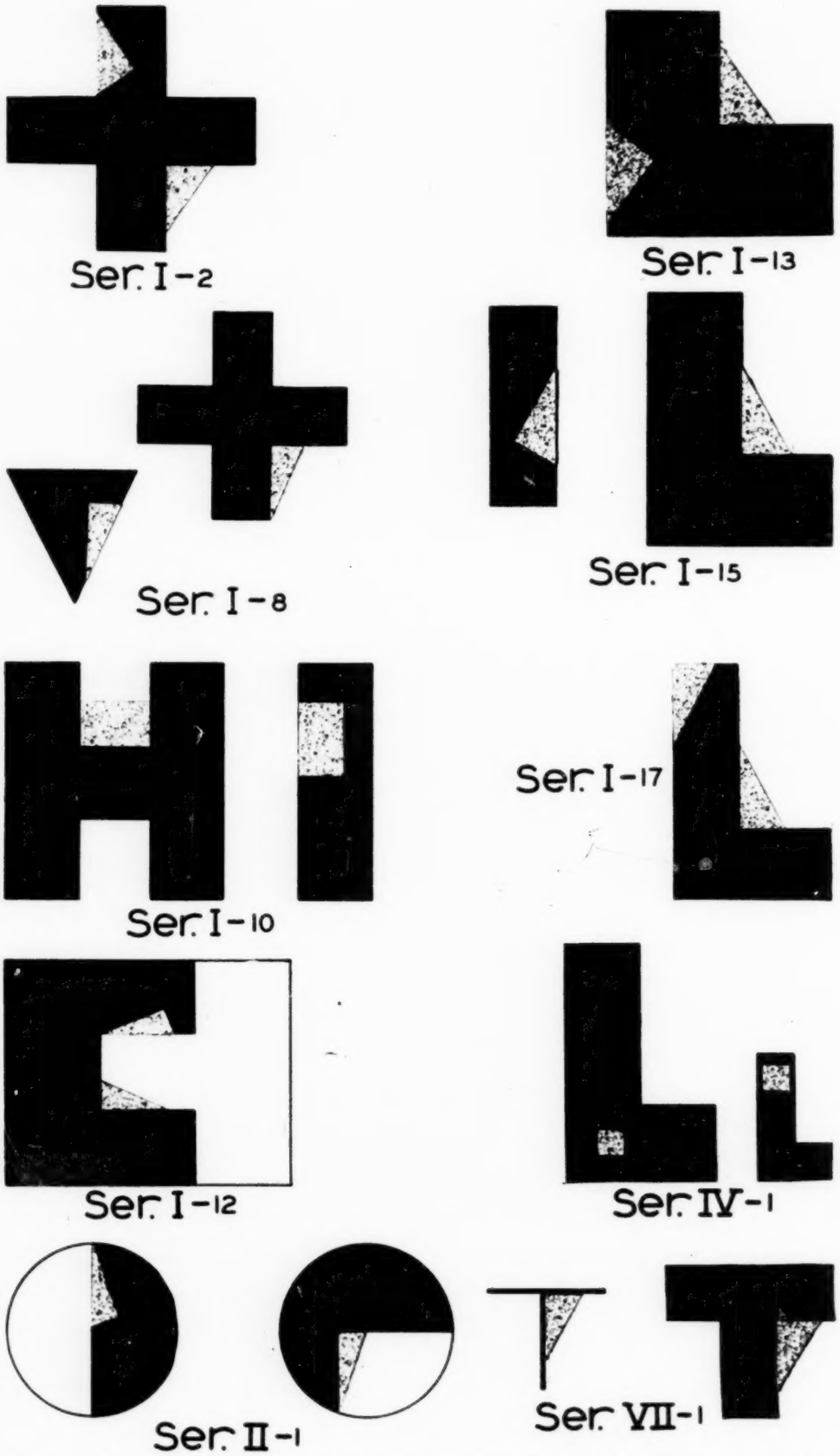

FIG. 1. Samples of the geometrical and literal designs used. (The gray patches were homogeneous and somewhat darker than they appear here.)

(*i*) A slight departure from the Benary type. G_i has one black (design) border and G_o has two. Should G_i be reported as lighter, this fact would indicate that an area into which a gray is inserted would have a greater effect upon it than a much larger area would have upon the outside gray. No. 17 was *black on white* (Fig. 1, Ser. I–17); no. 18 was *white on black*.

	Length	Width	Width of Horiz.	Crit. Grays
(17, 18).....	14·5	4 (top) 10 (bottom)	4·5	3·2 x 5 x 5·7

The observers[5] had an interval of at least a day between observations. One of the four orientations of the cards was given at each sitting. A position was not repeated under two weeks.

The order of orientation[6] throughout the series was as follows:

1st set	First position	Designs	1–18
	Second "	"	1–18
	Third "	"	3, 2, 1, 5, 4, 12–18, 6–11
	Fourth "	"	ditto
2d set	First position	Designs	17, 18, 1–16
	Second "	"	ditto
	Third "	"	12–18, 6–11, 1–5

The results of Series I are presented in Tables I and II, the first giving design by design and the last summarizing the

TABLE I

SERIES I: THE BENARY DESIGNS AND VARIANTS
Black and Dark Gray Designs on White Ground

		Lighter		Darker		Equal	No Report	Totals
		G_i	G_o	G_i	G_o			
	Crosses							
(1)	Large black.....	7	1	2	42	1	11	64
(2)	Large black.....	10	2	1	41	5	5	64
(3)	Large black.....	14	1	1	45	0	3	64
(4)	Small black.....	9	2	0	45	2	6	64
(7)	Large dk gray.....	1	1	1	12	13	36	64
(8)	Triangle-Cross.....	9	3	6	27	11	8	64
(10)	H-I Design.....	16	1	6	25	6	10	64
(12)	Double Design.....	8	0	7	30	5	14	64
(13)	Large L.....	13	1	5	36	3	5	63
(15)	I-L Design.....	12	0	4	31	10	6	63
(17)	Small L.....	6	6	12	27	9	2	62
	Totals.....	105	18	45	361	65	106	700

[5] Dr. P. T. Young (Y), Dr. E. A. Culler (C), Dr. G. D. Higginson (Hi), Dr. Elizabeth Möller (M), Dr. Alice Sullivan (Su), and Mrs. G. Sallee (S), of the instructorial staff of the Department; and Miss Ruth Franklin (F), R. Gundlach (G), W. D. Turner (T), and W. Zuschke (Z), graduate students.

[6] Hi and Z observed only the first four positions without any repetition of the four orientations. Y had only 2 positions (1st and 2d) for 4 and 10, and 1 position for 11.

White Designs on Black Ground

	Lighter G_i	Lighter G_o	Darker G_i	Darker G_o	Equal	No Report	Totals
Crosses							
(5) Large white	11	7	28	3	2	13	64
(6) Small white	2	4	45	3	3	7	64
(9) Triangle-Cross	5	0	27	3	7	22	64
(11) H–I Design	2	2	30	4	8	18	64
(14) Large L	4	7	39	3	7	4	64
(16) I–L Design	3	5	38	1	10	6	63
(18) Small L	5	3	22	15	12	4	61
Totals	32	28	229	32	49	74	444

entire series. Our 10 observers made 700 reports upon the black-and-gray patterns. If we subtract the 106 'no-reports' (Table I, above), we find that 465 of the remaining 594 reports suggest that the *outside*[7] critical gray (G_o) is phenomenally *darker* ($361\ DG_o + 105\ LG_i = 466$) than the inside critical gray (G_i). Only 63 (18 + 45) take the contrary direction. The corresponding results from the white-on-black objects stand 257 (229 + 28) for a *lighter outside* critical gray (G_o) against only 64 (32 + 32) in the contrary direction.

TABLE II

Reports for 10 Observers from 11 Black Designs on White

Obs.	G_oD	G_iL	G_oL	G_iD	Equal	No Report	Totals
C	8	36	11	1	9	12	77
F	51	0	1	8	7	10	77
G	49	4	1	8	6	9	77
Hi	3	31	1	0	7	2	44
M	49	5	0	8	9	6	77
S	60	1	0	5	5	6	77
Su	27	25	3	4	14	4	77
T	58	3	0	6	3	7	77
Y	24	0	0	3	2	44	73
Z	31	0	1	2	2	8	44
Totals	360	105	18	45	64	108	700
	465		63				

Reports for 10 Observers from 7 White Designs on Black

C	0	15	6	18	8	2	49
F	6	0	1	34	5	3	49
G	5	3	4	30	4	3	49
Hi	0	2	5	10	7	4	28
M	4	2	2	30	4	7	49
S	0	2	0	41	3	3	49
Su	5	6	8	18	8	4	49
T	10	0	0	30	3	6	49
Y	1	1	0	6	5	32	45
Z	1	1	2	12	2	10	28
Totals	32	32	28	229	49	74	444
	64		257				

[7] For the present, 'outside' will merely mean 'spatially outside.' It will not imply that the pattern is configured in the sense of the *Gestalt*.

The difference of the phenomenal (critical) grays appears with all ten observers and with the white as well as with the black designs on highly contrastive ground. The above results run contrary to what might be expected according to the ordinary effect of contrast.

Whether the relative difference from the black and the white designs (66 percent and 58 percent) is significant is by no means certain.

Special attention is called to the results from designs 13 and 14 (black L on white and white L on black ground). Each of these designs was a simple pattern and the design-areas around G_i and G_o were the same. It is seen from Table I that 49 (13 + 36) out of 58 reports (5 'no-reports' subtracted), for the first design, pronounce G_i lighter; and 46 (7 + 39) out of 60 reports (4 'no-reports' subtracted), for the second design, pronounce G_i darker. These results agree with those obtained from designs (such as Benary's cross) where there is more black around G_o than around G_i.

In patterns 17 and 18 (*B* on *W*, *W* on *B*) G_o has two design-area borders to one for G_i. Out of 60 reports on No. 17, 33 (6 + 27) represent G_i as lighter. Out of 57 on No. 18, only 25 (3 + 22) represent G_i as darker. We here find that reports on No. 17 indicate barely one half for G_i as lighter and on No. 18 less than half for G_i as darker. This result is obtained even though the G_i gray is *within* the pattern as much as it is in the other designs of the series.

In patterns where there is the same design-area about each gray (13, 14) we therefore get a result which strongly confirms Benary's; but in designs where there are twice as many design-borders about G_o as about G_i, the number of reports is small for the difference as compared with results from the other designs.

Summary of Series I. (1) Where we have one gray within and another without a geometrical design, with more black around one gray than the other, we obtain results which are not wholly explained according to the ordinary summational effects of contrasting brightnesses, although the latter are in evidence.

(2) The above conclusion also holds for designs where the relative amount of border upon the design-areas is the same for the two grays.

(3) If the contrastive borders of the design are decreased, so that the G_o gray has two while G_i has but one, then there is a decrease in the number of reports which run contrary to simple contrast.

To make sure that the critical grays were not themselves phenomenally different in their various positions they were observed as in the preceding series *but without the designs.*

They were presented in their four orientations (up, down, right and left) upon plain black and plain white backgrounds. Shape, size and relative positions were the same as before. The grays were from the large cross and the H–I figures.

Eleven Os (Dr. H. Helson was here added) made 208 observations on the black background and 208 observations on the white background. Of these grays, 121 (on black) and 134 (on white) were pronounced equal. Some Os reported 'different' more often than others and there was a general tendency to report 'darker' oftener than 'lighter'; but none of the gray pairs showed a difference between lighter and darker in any spatial arrangement, for any gray form, or with any of the four placements by rotation.

It appears therefore that the reported difference in the grays in Series I rests upon the presence of the designs and not upon position or orientation in the black and white fields.

Series II: Mutilated Designs. An attempt was here made to eliminate the Benary patterns but to retain the black and white borders of the grays. This was accomplished by including in a circular area the spatial context of the grays of Series I (Fig. 1, Ser. II–1).

For example, the G_i gray of Benary's cross was transferred, with its immediate surroundings of black and white, to a circular disc and the G_o gray to another of the same size. The relative amount of black and white borders of both grays was preserved for all the excerpts. These circular patches were too small to include the entire cross or any other original pattern. They were exposed upon the large white background. Five sets of designs were used. We were still left with a (circular) design, of course. In the excerpt from the cross, *e.g.*, G_i and its black and white borders were seen at times as a disc-shaped pattern with black, white and gray parts, and G_o with its context as a somewhat similar design. But at least the original geometrical and lettered patterns, assumed by the Gestalt psychologists to be configured with or without a given gray, were eliminated.

There were also four non-circular sets of excerpts from Benary's designs used merely because they were more feasible to construct than circular designs.

The Os were now asked to report not only upon the gray qualities but also upon the nature of their 'inclusion' or 'exclusion.' The gray-quality reports are presented in Table III. Of 290 reports (nine black excerpts on white), 191 (about $\frac{2}{3}$ of all) give the spatially inside gray as lighter. Out of a total of 346 descriptive reports, 275 show G_i as phenome-

nally 'inside'; while G_o was perceived as phenomenally within the borders only 45 times, and 23 of these 45 cases were from

TABLE III

SERIES II: MUTILATED FIGURES

All Reports of 11 Os from 9 Black Designs on White

Os	G_oD	G_iL	G_oL	G_iD	Equal	No Report	Totals
C	0	19	5	0	2	6	32
F	20	1	0	4	3	4	32
G	12	2	1	1	11	4	31
H	18	11	4	5	1	3	42
Hi	2	9	2	1	4	2	20
M	26	0	0	4	4	1	35
S	10	8	4	3	3	4	32
Su	7	18	1	0	9	3	38
T	19	0	0	4	8	3	34
Y	4	0	0	2	2	24	32
Z	1	4	1	1	9	2	18
Totals	119	72	18	25	56	56	346
		191		43			

two excerpts which were so cut as virtually to set G_o within the boundaries of the new design.

Comments of Os upon the inclusion and exclusion of the grays follow. (The initial of the observer is given.)

"The inside gray is just placed inside the black. There is no unitary effect with it." (C)

"When the gray is placed within the design I can't see that it belongs there; rather it appears as though it does not belong." (F)

"The gray which is placed into the black is just inserted there like a wedge in a block of wood. I get no integration." (G)

"I perceive no integration when a gray is placed inside an area." (H)

"These objects are disordered to me. There is no unity of the gray with the rest of the pattern." (Hi)

"These designs I dislike for they are so disordered. The inside gray does not appear to make for any kind of unity." (M)

"I shall be glad when this series is over for these objects are so badly formed. There is no unitary object." (S)

"It is very difficult to observe these designs because they are so disordered with no integration of parts." (Su)

"The inside gray does not unify with the pattern." (T)

"The grays to me are always apart from the designs: I get no unitary effect." (Y)

"The grays form no unity with the black or white. I want to snatch them away." (Z)

Summary of Series II. (1) The grays of the excerpts of Benary's geometrical and literal patterns were frequently perceived as one *within* and the other *without* these excerpts.

(2) The *inside* gray (G_i) is commonly lighter and the outside gray (G_o) darker, as in Benary's complete designs. But since Benary's patterns were obliterated, they cannot be held responsible, as he contends, for the observed difference in the grays.

(3) 'Nonsense' designs reproduce Benary's results when the inclusion and exclusion of the grays and their immediate context are maintained.

(4) The descriptive reports indicate that the gray was seen as 'inside' or 'outside' only in a spatial and local way, not belonging to it or appearing as a member in a unitary whole.

Series III: Cross Effects of Design and Background. Benary's chief interest in his 'figures' seems to lie only in the effect of the patterns upon his 'inside' grays. He has neglected the possible effect here of the background, as well as the influence of the main pattern upon his 'outside' gray. We should have expected that Rubin's 'figure-and-ground' concept, which he mentions (p. 140), would have suggested these relations; but evidently it did not. The purpose of the present series lies just in this direction, *i.e.* it is to test the effect of the background upon the 'inside' gray and the effect of the design-area upon the 'outside' gray.

To this end seven patterns were prepared, each showing side by side duplicate designs with an inserted gray (G_i) or an adjacent gray (G_o). In one design (right or left in the pair) contrast from figure or ground was diminished (1) by pasting a similar gray alongside G_i or G_o upon the background, (2) by making one design of a gray similar to G_o and the other of black, or (3) by making both designs slightly unlike an *inserted* gray (the left design) and an outside gray (right design). The designs were thick rectangular Is and Ls. The Os then reported the lighter (darker) of the two critical grays of the twin designs as these were presented side by side.

Our results make it evident that the effect of the background upon G_i and of the design upon G_o was greatly reduced when the critical gray was separated from background or design by a very similar gray strip. This rule held both for

black on white and for white on black. The ratios in the reports were 25 : 4, 22 : 5 and 21 : 7 ('equal' and 'no-report' thrown out) for our designs 1, 2, and 3. Pair 4 (gray and black twin-designs) showed again that the brightness of the design has a decided effect on G_o as well as on G_i. Ratio 24 : 3. It appeared, however, that when a high brightness difference was removed between design and the critical grays —two gray designs compared, one with (a slightly different) G_i and the other with (a slightly different) G_o—the background (whether white or black) exerted a greater effect upon the G_o than upon G_i. (Ratios 9 : 0 and 3 : 0, with 19 and 25 'equals.') This suggests that when the distinction between design and the critical grays is almost eliminated, the background exerts a greater effect upon G_o than upon G_i. The 11 Os were highly consistent in all these results.

When 4 Os, subsequently to this series, directly compared designs, one of which showed the *effect of the background upon* G_i and the other the *effect of the design upon* G_o, the two cross-effects appeared to be sensibly equal. This is a nice matter which deserves further study.

Summary of Series III. (1) The design-area has a decided contrast effect upon an *outside* gray.

(2) The ground has a decided contrast effect upon an *inside* gray.

(3) There appears to be a pronounced effect from all of the inducing fields surrounding each critical gray, whether the latter is spatially within or without the main pattern.

Series IV: Submerged G_i with Variable Amounts of Design and of Critical Gray. This series was designed to verify the contrast effect of the patterned area upon an internal gray member *which has no border on the background*, in order to see whether this member received the contrast effect according to the summational theory or whether a configurational or some other perceptive factor also was required.

Eight black designs (Ls, triangles and squares) were made with gray inserts (also Ls, triangles and squares) which were entirely surrounded by the design. Each type of design was presented in 2s, 3s or 4s, showing a difference in (1) size of

design, (2) size of gray insert, or (3) position of insert within the design. A variation in relative size or in position would seem to lead to a variable contrastive effect which might come out in the comparative reports upon gray quality.

Two sorts of apprehension were used. The Os were first instructed to observe the grays with no attention given to designs and position. Later they were asked to assume a 'totalization' attitude; to be set for the observation of the total pattern, and at the same time to be prepared to compare and to report upon the quality of the grays. The purpose of this instruction was to see whether the apprehension of the integrated 'figure' had an effect upon the gray quality. In the first series certain Os had reported that, upon the first exposure of the patterns, no designs were recognized as such—simply as blotches of black, white and gray—; but that the grays and their immediate contour patterns were clearly delineated. This analytical attitude was then held by F, M and Z. Gradually, under repetition, in Series I, the patterns were more and more definitely apprehended. The other Os commented on their synthetic attitude, which caught the entire design.

Under each instruction 327 observations (11 Os) were taken. Among these 654 observations (327 + 327) only 20 reports showed a discrepancy in the grays (as 'same' or 'different') as between the 'analytic' and the 'synthetic' attitudes. This fact testifies to the high reliability of the reports. It must not be hastily inferred, however, that the 11 Os always followed the instruction to ignore the 'total pattern.' It is one thing to give an instruction and quite another to discover whether it has been followed. As a matter of fact, only 4 Os (F, M, Su and Z) here reported an abstractive perception of the gray patch. They reported it in 76 out of a total of 108 cases under the 'analytic' instruction; while their 'total designs' were reported in every one of the 108 cases under the 'synthetic' or 'configural' instruction. It may be significant that 14 of the 20 discrepant reports were made by these 4 observers; though a slightly greater efficiency of observation and report under the one attitude

or the other might well have accounted for the discrepancies. The 62 (76–14) coincidences of report under the two attitudes of these 4 Os certainly does not strongly suggest that a configurative apprehension of these designs-with-insertions plays (under our conditions) any part in the determination of gray quality.

Comments of the 4 Os who had complete abstractive perceptions are here added.

"I did not see designs at all the first time" (referring to exposure with abstract attitude). "I could not sketch them; they seemed longish but I cannot tell what they were. I saw clearly the black and white immediately about the grays." (F)

"I saw no designs the first time—just a massed blur of black and the grays protruding in front." (M) (This O seemed very much surprised when designs were seen under the integrative set. She said, "The black appeared more striking and the area of black appeared much bigger in area: I can't believe that you are showing me the same thing.")

"The designs were not at all clear the first time. I could not possibly sketch them. In the totalization set the design is so prominent and startling that the gray appears very dim and faint. I compare them with great difficulty." (Su)

"In the first attitude I can hardly tell what the background is; much less tell its shape. Under the totalization attitude the designs seem smaller and farther away." (Z)

As regards plain contrast-effects, we may say that size and position would tend to make the *left* gray lighter in designs 1–6 and the *right* gray lighter in designs 7, 8. Our reports turn out as follows.

	Left lighter	Right lighter
Designs 1–6	276	151
" 7, 8	49	83

There is, then, a general confirmation of the contrastive effect.

Summary of Series IV. (1) There is no clear difference in the quality of the grays as observed in abstraction from the main design and with the whole 'figure' integrated. This result is concordant with our results with mutilated designs (Series II).

(2) A gray which is set wholly within the spatial limits of a design is amenable to the summational effects of contrast.

Series V: 'Inhering' Grays Spatially Outside the Design. Benary alleges that if a gray is 'included' within a design it is subject to a configurational influence. This alleged influence must mean more, then, than mere physical or spatial insertion

of the gray within a geometrical or lettered boundary. May the gray not actually stand apart from the pattern and still 'inhere?' We have tried in the present series to complete a configuration with the critical gray falling outside the spatial boundaries of the main pattern.

Our designs may be called *completion designs*. The horizontal bar of an L (*e.g.*) completes the literal character, while a like bar attached part way down on one side of a like vertical may not complete anything but only add another line. These two grays may, then, functionally correspond with the G_i and the G_o of our earlier series. Since we are dealing with an inherent membership instead of with a physical inclusion, however, we shall have to designate these grays as G_c (a completing gray) and G_n (a non-completing gray).

Let us see whether the quality of such an 'integrated' gray is explained according to the ordinary rules of contrast or requires the configurational or other comparable factor. The 11 patterns were always presented in pairs. On one member of each pair was a gray strip attached to black and *completing* the black design. The other member also had a like gray strip attached; but the black and gray together made (at least for the experimenter) no integral figure. Our completed designs were L, H, E, T, a sharp triangle set squarely upon its base, a round disc and a hat with its gray band. There were several variants of the literal designs.

We found it necessary, of course, to discover by way of the O's report what 'figure' was *actually perceived* and just *how it was integrated*. In displaying our gray comparisons, then, we shall distinguish the *conventional* designs from the *actual phenomenal* integrations of the observers.

We obtained 662 observations from our 11 Os. Of these a little over half (341) showed one of the two compared designs as actually completed by the gray, *i.e.* as phenomenally integrated. Of these 94 were 'equals' and 'no-reports.' This left 247 reports divided between 'lighter' and 'darker' in the ratio 189L : 58D (about 3.3 times as many L as D reports). This would seem to support the *Gestalt* doctrine that an incorporated gray tends to lighten, as in Benary's cross. But this

conclusion would be hasty. Let us examine the 321 of the 662 cases in which the presumably 'complete' design was not actually integrated by our Os. Here the ratio is 123L : 46D (about 2.7 times as many L as D reports).[8] The two ratios are not greatly different.

Summary of Series V. Although we found it possible (Series V) to complete a design by an 'outside' gray, it now appears that such integrations offer slight support for the alleged (Benary and others) lightening of the gray under configuration.

Series VI: Good and Bad Letters. The purpose of the present series was to examine still further the integrative factor in regard to its influence upon the gray quality of the critical field. The series was prompted by the results of Fuchs,[9] who found that the 'crucial' part of a configured object may exercise more of an effect than its size would warrant. Such a part is the middle horizontal bar of the letter E when colored differently from the other parts of the 'figure.' The color of this crucial part may have such an effect upon the total object that the color of the other parts will tend to be assimilated to it. The present series is designed to ascertain this effect upon the gray quality when a crucial part of the design has been misplaced. Here the general conditions are as in the preceding series; except that a misplaced 'inside' crucial part is set for comparison against another 'inside' crucial part which is well integrated. There is a need for the misplaced member in the completion of the pattern; but it lacks unity with the entire design because it is set out of its customary place. Let us see, then, whether a necessary but integrated gray is reported in respect to its quality in the same way as the 'outside' gray of the previous series.

Our three designs were 'good' and 'defective' black As, Es and Ls. The horizontal lines of A and L and the middle horizontal of E were like grays. In the 'defective' letters,

[8] The larger number of 'equals' and 'no-reports' here suggests that non-integration in both designs increased the difficulty of report.

[9] W. Fuchs, Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten, *Zsch. f. Psychol.*, 1923, 92, 298 ff.

the cross lines of A and L were set too high and the middle tongue of E too low. The good and defective As, set opposite each other, made a pair for the comparison of the gray horizontals, and so with the Es and the Ls.

The three 'good' letters were phenomenally integrated, including the gray horizontal, 305 times by all 11 Os. They were non-integrated (5 Os) 82 times (the A only 3 times). If we leave out 'equals' and 'no-reports,' the ratio for the integrated letters is 9.5L : 1D; for the non-integrated good letters 3.1 : 1. Thus the gray when integrated is reported 'lighter' about three times as often as the same gray when not phenomenally bound into the literal design. As for plain contrast, that would always tell in the opposite direction, because the misplaced gray member is here closer to the black and should therefore be lighter. That makes the matter of integration a little difficult to determine; for of the two 'disintegrated' As ('good' and 'bad') we should expect the 'bad' (*i.e.* with misplaced gray tongue) to be always lighter. But we have seen that it is not!

Summary of Series VI. A horizontal gray bar of a properly fashioned A, E or L strongly tends to be lighter than the same gray bar when misplaced and the tendency is stronger when the properly fashioned letter is well unified. Brightness contrast would lead us to expect the opposite result. Whether the 'correct' letter is a real case of *configuration*, it is difficult to say. It seems rather to be a matter of a conventional letter taken as a symbol.

Series VII: Heavy and Skeletal Designs. Here we examine designs which have outside grays attached to them which are not only spatially excluded but also excluded as unitary or essential parts or members. The need of including such patterns arose from Series III, where it was observed that the design-area has a pronounced effect upon the 'outside' gray.

Our first two patterns were a T and an H. Two Ts were presented in a pair, one very thick and black, the other a thin-lined skeletal T (Fig. 1, Ser. VII–1). Filling the angle at the right (under the arm of the T) was a gray triangle photometrically the same for the thick and thin Ts. A thick black H and a

thin skeletal H made up the second pair. Here a gray rectangle filled in slightly more than half the middle space above the cross bar. Both grays were spatially and phenomenally outside the designs.

Now since designs influence outside grays (Series III) we might expect that the two Ts or the two Hs (being like configurations) would exert a like effect on the grays. But as black masses, the contrast from the heavy designs would naturally make their attached grays lighter than the skeletal designs would. And they apparently did, for our results stand

	Lighter	Darker
Heavy Black T	35	3
" " H	33	10

Our second type of pattern was a plain heavy black cross with like gray triangles set adjacent to the lower right and upper left quadrants. The lower gray fitted the angle, touching the black cross on two sides, the upper touched only along its hypotenuse. If the amount of black-gray contour counted, then the lower gray should be seen as lighter. But as a matter of fact the upper was more frequently reported lighter.

	Lighter	Darker
Upper gray	96	30

If this result stands against black-white contrast, it is no easier to explain it by configuration, since *both grays* stand outside the phenomenal design. The only clue which we have is that the upper gray triangle is more *obtrusive* and, so to say, *unfitting* and *detached*. But what effect (if any) this perceptive difference might have we are unable to say. The result stands squarely against Benary's rule that the detached (G_o) gray is the darker.

Summary of Series VII. (1) The amount of *body* of the design obviously affects adjacent grays by way of contrast; but the configuration does not seem to count.

(2) Of two like outside grays the more detached is reported lighter. This stands against Benary's law of influence.

Series VIII: Designs without Contrasting Grounds. i. Like design and ground.—It occurred to us that if we could use

black designs on black grounds (thus eliminating contrast at the contours), we might get a 'purer' configurational effect. This we managed by suspending the black cross with G_i and G_o before a like black ground. The difference in distance made the cross integral. But now there was no clear difference between G_i and G_o. The results for 11 Os follow.

G_i Lighter	G_o Lighter	Equal	Total
8	4	76	88

When the black cross was pasted upon a just-different black ground and G_i was configured, the results likewise ran

G_i Lighter	G_o Lighter	Equal	Total
7	10	25	42

In both cases with black-white contrast gone but with configuration of G_i left, no significant change in the grays appeared.

ii. Like critical grays and ground.—Nevertheless the magical influence of the black patches did appear when a gray ground (indistinguishable from G_i and G_o) was substituted for the black ground. G_i and G_o were contoured against the like gray ground by a fine black line. The figures stand (4 Os)

G_i Lighter	G_o Lighter	Equal	Total
27	1	4	32

The difference is striking. Possibly the comparison is facilitated and discrimination sharpened by the juxtaposition of the grays.

iii. Similar dark design and ground with light G_i and G_o.—If the tempering of G_i depends (as Benary and others maintain) upon the configuration of it with the design, then we should expect the tempering to occur *whatever the background.* Limited experiments with the critical grays lighter than design and ground gave us no positive evidence of this tempering. The point is important and should be pressed further.

Summary of Series VIII. The effect of removing contrasting surfaces depends upon whether the like members are design-and-ground or critical-grays-and-ground. The first condition tends to equate G_i and G_o; the second to make G_i lighter on a black design. Where G_i and G_o are lighter than design and ground, no fixed difference in them appears.

Discussion and Conclusion

We have found occasion to criticize the methods and the logic of the doctrine of *Gestalt* where that doctrine has sought experimentally to refer to 'configuration' the observed qualitative change in a gray insert in a black or white design. In our experiments we have repeated and extended, under stricter control, the observations of Benary and others. We have also tried to maintain a sharp distinction between geometrical designs and spatial arrangements, on the one hand, and alleged inherence and configuration, on the other.

The more significant of our experimental results may be now set down.

In the original Benary designs, a gray patch (G_i) spatially included within the contours of the black (or white) design tended to be reported as lighter (or darker) than a photometrically similar gray (G_o) set just outside these contours. So far there is a factual agreement with Benary.

By comparing the grays on a common field but without the black (or white) design, we made sure that the above outcome was not due to placement, distance or orientation of the grays, for under these conditions they tended to be the same in quality.

When we destroyed Benary's designs (Series II) by limiting the compared areas to the grays and their immediate surroundings (method of mutilated excerpts) we obtained the same result as before. Furthermore, G_i was reported as 'lighter' when the black, white and gray were apprehended as *juxtaposed patches* and not *configured* in Benary's sense, when G_i was within the black (or white) in a *spatial* way but not integral to, or *membered* with, it.

If our mutilated excerpts are 'configurations,' they seem to violate almost every property recognized in the description of the *Gestalt*. They are neither simple nor unequivocal but indefinite and chaotic; and if they possess the *Prägnanz* of Wertheimer (cf. footnote 2), it is the *Prägnanz* of poor and loose structures. This lack of order or integration of parts is very much against Benary's contention that inherence of a part is necessary to obtain a lighter quality of the inside gray as against simple contrast.

We next discovered cross-effects (Series iii). The design qualitatively affected G_o, and the background affected G_i. This stands against Benary's belief that the design (confused with 'figure' in the *Gestalt* sense) 'determines' only the G_i member.

These results call for a more careful definition of Köhler's notion that the 'figure' possesses a greater 'density of energy' than the 'ground.' Were this true we should expect a high resistance of the figure against the background; a resistance for which we find little evidence in our results. We should also question other exponents of the Gestalt who agree with Köhler in the treatment of memory-color. Again, Rubin found that shadows are better seen on ground than on figure and that the contours belong to figure and not to ground; Koffka maintained that retinal rivalry is very different for figure and for ground, that phenomenal figures have boundary lines even when the corresponding objective figures have none, and that contour makes for a 'closed' figure, independently of the ground; Schumann asserts that boundary lines separate the figure from the ground; and, finally, Gelb and Granit contend that figure has a higher color-limen than ground. All these writers very clearly imply that figure regarded as a 'thing' has a greater density of energy and therefore a greater resistance than the ground and that the figure is a 'closed' system in respect to the ground. But if observation shows a free intercourse between what is called 'figure' and what is called 'ground,' then we feel compelled to ask the configurationists to define carefully what they mean by the figure as a 'closed' system. Our results do not reveal any appreciable barrier between a 'closed' figure and its surrounding background.

When we wholly submerged G_i in the design (Series iv), we found it to be amenable to brightness contrast; but we also found the gray quality to be the same whether 'membered' with the design or observed in abstraction from it.

When we succeeded in completing a design (Series v) by a gray that was spatially outside it, this completing gray tended to be lighter than a photometrically similar gray which failed to complete. But the lightening appeared practically the same with and without configuration.

A gray horizontal bar completing the letters A, E and L appeared (Series vi) lighter when it was seen to form a 'good' letter than when it 'distorted' the letter, and this effect stood against areal contrast. This is not a plain case of configuration, though the result is difficult of interpretation.

When G_o is set against heavy and skeletal designs (Series vii) it is plainly modified in accordance with the black masses adjacent to it (areal contrast); but configuration is not found to play a part.

The removal of contrastive surfaces (Series viii) as between design and ground tended to equate G_i and G_o, although the design and presumably the 'configuration' remained; but when G_i and G_o were separated by a thin contour line from a like gray background, G_i tended to be lighter. Critical grays (G_i and G_o) lighter than the design and background showed no alleged effect of configuration but remained the same gray.

Our general conclusion is that if 'configuration' is to be used as an explanatory term it must be attested and not either assumed or confused with geometrical or symbolic designs. We find in our own experiments, as well as in those of Gestalt psychology, that *at most* this alleged factor *slightly modifies* gray quality. Why it should modify in the observed direction is not made clear even in the notion of 'accent' or 'sharpening.' There is no justification for the claim that 'the whole *determines* the parts.' In our case, the gray quality is *primarily* set by radiant energy locally exciting the retina; although it may be slightly modified (as has long been known) by various other factors. One obvious factor is the total retinal pattern of excitation. The modifications thus produced are usually set down under induction and contrast. They are obvious and effective in the case in hand. It is fairly obvious that the traditional 'laws of contrast' are not final. It also appears from our experiments that other and unnamed modifying conditions are present in some of our observations. But we find the hypotheses of configuration and inherence to be neither clear, consistent nor adequate in the present case of the modified gray quality.

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PERCEPTUAL DETERMINANTS IN PLANE DESIGNS

BY JOHN G. JENKINS

Iowa State College [1]

In recent years there has been a growing tendency to question Hering's retinal theory of contrast and to insist that more than mere peripheral spread is involved. Thus Tennant [2] has published experimental results from which he infers that both physiological (peripheral) and psychological factors are operative in simultaneous contrast; and the proponents of *Gestalt-Theorie* have furnished many experimental demonstrations that central factors must be considered in any adequate account of such contrastive effects. Fuchs,[3] rejecting Wundt's explanation of *Florkontrast* [4] in terms of an associative interaction, insists that configural influences are responsible for the effects obtained; and Benary [5] was constrained by his experiments with black and white designs to conclude that "the contrastive effect of the figural part of the field is not to be defined solely in terms of the quality, nearness and amount of the inducing parts of the field, but consideration must be given as well to the *Gestalt*-totality of which this particular area is part" (p. 141).

Benary reports certain qualitative results which should be noted here.

(*a*) When he exposes black (or white) designs on white (or black) backgrounds, two gray patches (within and without the design) appear different in brightness.

(*b*) By using other geometrical designs, he is able to show that the effect described is not dependent upon the use of Wertheimer's original figures.

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[1] From the laboratory at Cornell University. The research was conducted under the direction of Professor Bentley.

[2] J. Tennant, The psychological factor in color contrast, *Brit. J. Psychol.*, 1929, 20, 1–26.

[3] W. Fuchs, Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten, *Zsch. f. Psychol.*, 1923, 92, 298.

[4] W. Wundt, *G. d. physiol. Psychol.*, 6 Aufl., 1910, 2, 267.

[5] W. Benary, Beobachtungen zu einem Experiment über Helligkeitskontrast, *Psychol. Forsch.*, 1924, 5, 131–142.

(*c*) Variation of the brightness-step between figure and background has but little effect upon the difference between the grays.

(*d*) The effect is relatively independent of the distance between the observer and the exposed design.

(*e*) The difference between the grays appears upon successive as well as simultaneous presentation.

(*f*) It is apparently independent of the direction and character of fixation.

(*g*) The effect is increased by covering the design with tissue paper.

(*h*) An increase in the size of the critical fields produces no marked effect.

(*i*) When observers are instructed to reverse the figure-ground relationship in the ambiguous designs, they note no change in the relative brightness of the grays.

(*j*) Critical fields of the same brightness as the figure are taken as deformations of the figure.

(*k*) Critical fields of the same brightness as the ground disappear into the ground or exhibit traces of instable coloring.

(*l*) When the critical fields are darker than either figure or ground, the excluded patch is seen as brighter.

(*m*) Brightness differences appear when the configurations are presented in the form of pure outlines.

(*n*) When a blue figure on a yellow ground is shown, the observers report the excluded gray as darker and bluish, the included gray as lighter and yellowish.

On the basis of these results, Benary concludes that simultaneous contrast cannot be explained entirely in terms of peripheral spread of excitation. As he sees it, the results given in (*i*) show that this is not a simple matter of figure-and-ground, but rather a case in which the *Zugehörigkeit* of a gray patch to its figure acts as a determiner of its quality.[6]

To the four criticisms of Mikesell and Bentley[7] the present writer adds the following:

(5) As Benary gives no photometric values for the blacks and whites which he employs, his quantitative results must be regarded as instructive regarding the qualitative aspect of the problem rather than as significant for a quantitative comparison of the two critical fields.

(6) Benary concludes that inherence to figure, rather than any figure-ground relationship, is responsible for the effects obtained; yet his only basis for such an interpretation is to be found in a single experiment with the 'ambiguous' designs. The brightness difference between the gray patches remains unchanged, regardless of whether his observers take a particular area as figure or as ground. He says nothing of the number of observations made, nor of the number of observers who report in this fashion. In view of the 'figural after-effect' noted by Rubin[8] there may be serious doubt that his observers were able to make the change in attitude anticipated by his instructions.

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[6] We have been unable to find a satisfactory translation for *Zugehörigkeit*, which has been rendered as 'inherence' throughout this report. This fails, however, to give completely the idea of 'belonging-to-ness' implied in the German term.

[7] See Mikesell & Bentley, pp. 2–3, above.

[8] E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 6.

(7) As Benary offers no explanation of any influence that would make the included gray appear lighter, we may wonder at his ready acceptance of the appearance of the same gray as darker when white figures on black backgrounds are exposed. His statement that the inherence of a given patch must be considered as a contrast-determiner does not explain why a particular gray should appear lighter or darker. To complete the account this explanation must be sought.

The experiments now to be reported extend the investigation of Mikesell and Bentley. They will better control fixation, illumination, the state of visual adaptation, and the conditions of exposure. They also seek to throw new safeguards around the reports of the observers and to improve the formal instructions.

The Experiments

Apparatus. The apparatus employed was a modification of the mirror tachistoscope which was designed to provide an enlarged field of exposure, a simplification of parts and an increased ease and accuracy of control.

A transparent glass plate set across the main chamber at an angle of 45° served as a mirror when a lateral compartment containing a fixation screen was illuminated but permitted direct transmission of light when the lateral chamber was dark and the main or exposure chamber lighted. Fixation and exposure were timed by means of a fall apparatus which actuated a relay controlling the 110-volt current for the lamps. In the exposure chamber a single 60-watt daylight lamp gave satisfactory illumination. Absorption by the glass plate required the use of four such lamps in the lateral chamber in order to obtain approximate equality of lighting before, during and after the exposure. The designs themselves were mounted upon cards 14 in. x 22 in. For convenience' sake, black designs mounted upon white backgrounds will be referred to as BkW designs; and white figures upon black backgrounds will be designated as WBk designs.

Procedure. The entire investigation was conducted in a lightless room, in which a 15-watt bulb was used by the experimenter to illuminate the fall apparatus and to allow the recording of results. As the observer's head was screened by the apparatus, this light never appeared within his field of vision. Observations were made upon each series of designs over a period of about fifty minutes, and these observational periods were separated by at least twenty-four hours. In no case did any observer make more than six reports upon any one figure during a single period of observation, and in general about sixty observations were made during the entire period, a short rest-period being allowed after each complete series of designs had been exposed.

Exceptions will be noted as they occur; but the following may be regarded as typical of the experimental procedure. As O adjusted his head comfortably in the head-rest, he could see only the homogeneous white field of the fixation screen, bearing in its center the faint gray fixation-point. After a warning signal, E released the fall hammer, obliterating the fixation screen, and, at the same time, exposing the design for 500 sigma. This time was long enough to allow O to report with fairly high assurance, but short enough to prevent adaptation or gross eye-movement. Both fixation and exposure screens were at a distance of two meters from the head-rest.

While certain of the series required special instructions, the following were used in all series except those where variations will be described.

"When the signal 'ready' is given, fixate steadily the gray spot. At 'now,' continue your fixation, but observe attentively the objects presented. Compare the small gray patches as to brightness and report either

'Left (right) brighter'
'Same in brightness' or
'No report.'

In addition, designate your degree of assurance by one of the following numbers

4 for high assurance
3 for moderate assurance
2 for small assurance
1 for assurance wanting.

Avoid any set or predisposition. There is never any assurance that the grays will appear different at a given exposure. Be sure to report upon what you actually observe. Remember that 'No report' has exactly as much scientific value as reports of high assurance.

Report promptly and avoid complication of your task, which is simply to compare the brightness of the grays. Do not discuss your results with anyone. The exposure will be brief. Watch for the 'ready' signal."

Observers. The regular observers for the entire course of the experiment were Dr. S. Feldman (Fe), Dr. E. Freeman (Fr), Dr. G. Kreezer (Kr), and Miss E. Lowenstein (Lo).

The first three were members of the staff and were thoroughly trained in psychological observation. The last-named observer was untrained. For the special observations in Series F, four graduate students acted as observers; they were Messrs. O. D. Anderson (An), A. D. Glanville (Gl), W. T. James (Ja) and E. H. Kemp (Ke). All observers worked in ignorance of the problem under investigation.

Series A. This series was designed to test the phenomenal equality of two patches of the standard gray (Hering No. 6 paper used throughout the experiment) when these patches were free from any difference in location with respect to the design. With this end in view, tests were made with two designs. In the first design both patches were set within the boundaries of a circular black form: in the second both lay outside of the periphery of this black figure. The whole

design could be rotated so that the figure could be exhibited in each of the four major orientations. Each observer made twenty-five reports upon each design in each position, presentation being in haphazard order. In both figures the grays were reported as being the same in brightness slightly more than 50 percent of the time, while the remaining cases showed almost perfect chance distribution. There was little evidence of any preferred position as judged from reports of relative brightness, and, allowing for a space error, it is safe to say that the two grays appear equal in brightness when both are included within or excluded from a black design on a white background.

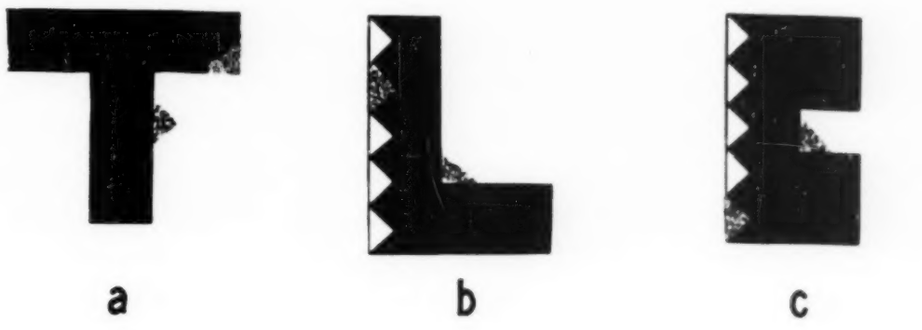
Series B. The second series involved the use of the designs employed originally by Benary; these were exposed under the controlled conditions described above in order further to verify the results obtained by Benary and by Mikesell and Bentley. The designs were constructed according to Benary's specifications, and were exhibited in upright and inverted positions. (As the results showed clearly that the effect reported by Benary overshadowed completely any positional effects, this procedure was abandoned and later series were presented only in the upright position.) Each observer made twenty observations upon each of the designs with results as given in Table I. In this, as in all succeeding tables, results are given only for the BkW forms of the designs: in cases where the WBk forms were employed as well, results for these will be summarized separately.

TABLE I

Obs.	Included Gray (G_i) Brighter	Excluded Gray (G_o) Brighter	Same and No-Report
Fe	65.0%	1.7%	33.3%
Fr	86.7%	8.3%	5.0%
Kr	72.5%	3.3%	24.2%
Lo	56.7%	10.0%	33.3%
Average	70.2%	5.8%	24.0%

These results demonstrate in no uncertain fashion that the two grays are reported consistently as different in brightness, and that this difference is in the direction indicated by Benary.

In the WBk designs, the situation is almost exactly the reverse of that shown above, as the included gray is selected as brighter only 1.9 percent of the time, while the excluded gray is reported as brighter 68.5 percent of the time. Here is a clear-cut vindication of Benary's qualitative results and an apparent substantiation of his claim that more than mere peripheral spread of excitation is involved in these cases of contrast. The two patches have always identical contrastive



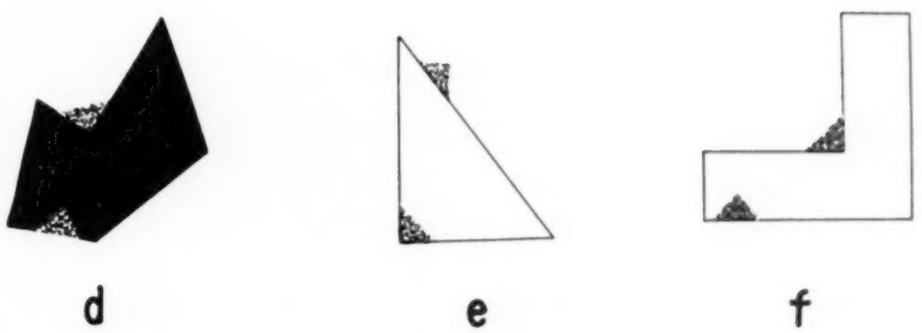


FIG. 1.

borders; and, on Benary's own admission, we may ignore any differences in induction-areas geometrically more remote (noting, however, that these areas, if effective, should tend to produce results opposite to those obtained in this series). In spite of this apparent equality of influences, the two grays are reported consistently as different in three separate investigations. It appears, therefore, that we must accept this state of affairs as definitely established and proceed further to test the conclusions set forth by Benary.

Series C. Benary explains the difference between the two grays in terms of the higher inherence in the figure of the included gray. Inspection of his designs (of which Fig. 1, a is an example) will show that the included gray patches were mere adventitious inserts into the body of the figure. If inherence, rather than mere spatial inclusion, is responsible for the effects obtained, it should be possible to decrease or increase the effect by varying the extent to which the included patch 'inheres' in the design. An attempt to produce designs which would show marked differences between the inherence of the two patches was made in this series. The sample (Fig. 1.b) will show that the included patch is actually necessary for the completion of a geometrical figure, while the excluded patch bears no such relationship. Now if *Zugehörigkeit* is involved in the difference between the two grays, we should expect an increase in the percentage of cases in which the included gray is reported as lighter. The results for seven designs of this sort (Table II), upon each of which the observers made twenty reports apiece, show that this is exactly what occurred.

TABLE II

Obs.	Included Gray (G_i) Brighter	Excluded Gray (G_o) Brighter	Same and No-Report
Fe	94.3%	2.1%	3.6%
Fr	97.1%	2.1%	0.8%
Kr	96.4%	0.0%	3.6%
Lo	88.6%	1.4%	10.0%
Average	94.1%	1.4%	4.5%

The situation predicted has indeed materialized, and the average of the four observers shows better than 90 percent of their reports favoring the included gray as brighter, while the contrary reports drop to about 1 percent. This is in apparent agreement with Mikesell and Bentley's discovery that a 'necessary' gray is reported as lighter than an 'unnecessary' gray; and the high stability of the phenomenon affords a further demonstration of the fact that explanation in peripheral terms alone will be difficult. A very natural check on the validity of these conclusions consists of an attempt to *decrease* the 'inherence' of the included patch to see if this will result

in a decrease in the frequency with which this gray is reported as lighter. This was precisely the task undertaken in the next series.

Series D. The designs in this series were so constructed that the included gray, while spatially within the boundaries of the black figure, constituted an interruption to the regularity of some part of the design (Fig. 1, c). It was hoped that this would make the included patch less inherent, and, in some cases, perhaps an actual intrusion which would be non-inherent. Ten such designs were employed, and each observer made twenty observations upon each design, with results as given in Table III. To bear out the results obtained in the last series, the included gray should now be chosen less frequently as lighter, with possibly an increase in reports of the excluded gray as lighter.

TABLE III

Obs.	Included Gray (G_i) Brighter	Excluded Gray (G_e) Brighter	Same and No-Report
Fe	38.0%	27.5%	34.5%
Fr	25.0%	58.0%	17.0%
Kr	24.5%	57.0%	18.5%
Lo	30.5%	45.5%	24.0%
Average	29.5%	47.0%	23.5%

The results are plainly in agreement with the requirements laid down above. Not only do the results indicate a great decrease in the percentage of reports favoring the included gray as lighter, but actually all of the observers except Fe show a complete reversal of the direction of choice, reporting the included gray as lighter less often than the excluded gray. These results must be taken to strengthen the position established by the last series. It is increasingly evident that perceptual factors of some sort must be considered in any explanation of these particular cases of visual contrast. The critical reader may object that this change in the frequency of reports favoring the included gray as brighter is due entirely to the decreased amount of black near that patch. The defense against this criticism is twofold. In the first place, the two patches still have identical black and white borders, and the

earlier series indicate that this condition seems to out-weigh effects of more remote differences in inducing areas. In the second place, if nothing more than these remote areas is the cause of this decrease, we should expect to find no change in the ratio of reports if we expose the designs of this series without the enclosing black line across the saw-teeth. On the other hand, if perceptual factors of some sort are effective here, we should expect to find that omitting this line has an observable effect upon the results. To answer this question, the designs were presented without the enclosing black lines. The results seem to support the conclusion that perceptual factors are concerned in the results of Series *D*, for the reversal noted in that series is not obtained here; now 39 percent of the reports refer to the included gray as lighter, while only 29 percent give the excluded gray as lighter, in contrast to the 29.5 : 47 ratio of Series *D*.

Series E. As it had been observed that the size of the critical fields appears to affect the stability of the phenomenon, it seemed wise to adopt a critical field of standard size for further investigations, in order to be rid of the variable of size. Earlier results pointed to a triangle 1.5 cm x 1.5 cm x 2.1 cm as satisfactory, and this was adopted. To study designs of high- and low-inherence without the variable of size, a new series was constructed, consisting of five designs of high and five designs of low inherence in which the critical fields were

TABLE IV

Obs.	Included Gray (G_i) Brighter		Excluded Gray (G_o) Brighter		Same and No-Report	
	High	Low	High	Low	High	Low
Fe.....	76%	38%	6%	26%	18%	36%
Fr.....	98%	26%	0%	60%	2%	14%
Kr.....	98%	24%	0%	32%	2%	44%
Lo.....	98%	14%	0%	44%	2%	42%
Average	92.5%	25.5%	1.5%	40.5%	6.0%	34.0%

all of the standard size described above. Each observer made ten reports upon each design. In addition to reporting upon relative brightness of the grays, he was instructed to "report each time upon the relation of the gray patches to

the black objects." For these reports a dictaphone was placed at the observer's side. All of the observers quickly became adapted to its use and gave spontaneous reports which could be accurately transcribed. The results of the series are given in Table IV.

These results offer striking support to the results of the last two series. In the designs of high inherence (referred to in Table IV and elsewhere by the term 'high') three of the observers select the included gray as brighter in 98 percent of the cases, without a single contrary choice; and every one of these observers shows a complete reversal of the direction of choice for the designs of low inherence ('low' in Table IV).

It is now time to turn to the reports upon the relationship between the gray patches and the black designs. While no clue in this direction had been given in the instructions, Benary's work would lead one to expect that various synonyms for '*Zugehörigkeit*' were due to appear in a majority of these reports; and this was found to be the case. In addition, careful inspection revealed certain other uniformities in descriptive terminology which seemed to hold out high promises for further investigation. As these uniformities supplied the motive for certain of the later series, it may be well to note at this point some of the more characteristic delineations of the relationship between the figure and the two patches in the high-inherence designs.

Characteristic Descriptions
of

Included Gray (G_i)	Excluded Gray (G_o)
Lighter	Darker
Belonging to figure	Not belonging to figure
Part of figure missing	Adjunct to figure
Hole in figure covered by a mist	Continuation of figure itself
Fragile, filmy, soft	Compact, surfacy, hard
Indistinct	Prominent, outstanding
Had to be sought	Emerged at once

The above are characteristic descriptions appearing throughout the series. Next in frequency only to the syno-

nyms for 'belongingness' is the reference to the 'prominence' of the excluded gray patch. An attempt to discover the nature of this 'prominence' was made in the next series.

Series F. The ten designs constructed for Series *E* were employed again for this series. The most obvious course seemed to lie in having the regular observers report upon the relative prominence of the two grays; but this was abandoned for two reasons. In the first place, the regular observers had made several hundred observations in which they had been instructed to attend to differences in brightness between the two patches. In view of our criticism of Benary for his reliance upon instruction to change an attitude of long standing, it seemed unwise to pin too much faith in the ability of our own observers to make such a change. In the second place, it seemed highly desirable to prevent the regular observers from forming any preconceptions as to the nature of the phenomenon upon which they were reporting. With these two points in mind, we decided to select four special observers to report upon this particular relationship. They were instructed to "compare the two grays as to relative prominence ('outstandingness,' 'importance')" and, in addition, to "explain in each case what you mean by saying that one patch is or is not more prominent than the other." The results in Table V represent ten reports upon each design by each observer.

TABLE V

Obs.	Included Gray (G_i) Prominent		Excluded Gray (G_o) Prominent		Same and No-Report	
	High	Low	High	Low	High	Low
An.....	48%	36%	48%	40%	4%	24%
Gl.....	0%	2%	96%	44%	4%	54%
Ja.....	8%	18%	88%	44%	4%	38%
Ke.....	6%	6%	88%	46%	6%	48%

Without the aid of the reports dictated by these observers, we could say only that three of the observers reported the excluded gray as more prominent in the vast majority of the high-inherence designs, while this figure dropped very markedly for those of low inherence. The apparent inconsistency of the fourth observer appears all the more disturbing

in view of the surprisingly close agreement between the other three observers until we look for explanation to the descriptions these observers have given. The three observers who show close agreement make almost no reference to brightness as a determinative factor in their reports on 'prominence.' An, on the other hand, continually reports a 'struggle for dominance' between the 'protrudingness' of the excluded gray and the 'greater contrast with its background' of the included gray. It is not surprising, under these circumstances, that his reports should show almost perfect chance distribution, indicating, apparently, that the two factors named above contributed about equally in determining the direction of his reports. The other observers agree in contributing data which seem to indicate a strongly negative relationship between the brightness of a patch and its prominence; thus, in the high inherence designs, we find that the included gray patch is chosen as brighter more than 90 percent of the time, while almost the same percentage of reports indicates that the excluded patch is the more prominent.

A test of the validity of these conclusions was provided at the end of the last series upon relative brightness. As the regular observers would not have to return to comparing the brightness of the grays after that time, it seemed possible that the opportunity to verify the results obtained by the special observers would outweigh to some extent the defects of such a procedure. The regular observers were accordingly instructed to report in terms of relative prominence, dictating explanations of their judgments during two of the ten observations made by each observer upon each design. The results of these observations are given in the following table.

TABLE VI

Obs.	Included Gray (G_i) Prominent		Excluded Gray (G_e) Prominent		Same and No-Report	
	High	Low	High	Low	High	Low
Fe.....	14%	14%	62%	78%	24%	8%
Fr.....	12%	32%	82%	22%	6%	46%
Kr.....	76%	32%	6%	50%	18%	18%
Lo.....	2%	2%	88%	64%	10%	34%

At first glance, these results may appear quite unfavorable to a verification of those obtained by the special observers;

but again the descriptions dictated by these observers come to our aid. Lo and Fr, it will be noted, give results comparable to those obtained by the special observers, showing a high percentage of choices for the excluded gray as more prominent in the high-inherence designs, and a lower percentage for this gray in the low-inherence figures. In the case of Kr, however, this situation is exactly reversed,—a baffling case until the perusal of his descriptions shows that he reports consistently a temporal change in his perception of the designs not reported by the other observers. Initially, he says, only the excluded patch is apprehended, but this patch loses in relative prominence as the other patch 'emerges.' Kr reports on this final stage; the other observers either do not experience this shift, or else they report upon the first stage of the process. If this investigation is to be concerned with the phenomenon on a primitive level, we may dismiss Kr's data with the statement that they seem to support rather than to refute the conclusion indicated by the reports of the other observers. There is nothing in Fe's reports to show why his results should be at variance with those of the other observers; though there is some reason to believe that his apprehension of the designs varied from that of his fellows, since his data tend to differ slightly from theirs all the way through the experiment.

Before accepting the results of these series as indicating that the excluded gray tends to be more prominent for some reason other than that of difference in apparent brightness, it is still necessary to perform a critical experiment. There is still a possibility that the difference in brightness may be more primitive, and that the excluded gray is more prominent simply because it appears darker. The test of this is, of course, to present the same designs in the WBk form, in which the excluded gray has been reported consistently as lighter. Ten such designs were constructed and each of the special observers made ten reports upon each design with the following results.

As An's reports still contain continual references to the 'struggle' between the protrusion of the excluded gray and the greater contrast of the included gray, we may disregard

his results. The data from the other three observers are in clear-cut agreement, and, as the excluded gray is still chosen as more prominent in a large majority of the cases, they constitute a good demonstration that factors other than

TABLE VII

WHITE DESIGNS ON BLACK

Obs.	Included Gray (G_i) Prominent		Excluded Gray (G_e) Prominent		Same and No-Report	
	High	Low	High	Low	High	Low
An.....	86%	56%	6%	16%	8%	28%
Gl.....	8%	2%	82%	32%	10%	66%
Ja.....	16%	12%	72%	40%	12%	48%
Ke.....	4%	16%	88%	34%	8%	50%

difference in apparent brightness must be responsible for the greater 'prominence' of the excluded gray. In spite of the fact that it is now perceived as brighter, the excluded gray is still chosen as more prominent in a large percentage of the cases with the high-inherence designs, the value dropping as before in the figures of low inherence. The reports show that the excluded patch is prominent and stands out both physically and experientially. This is a point to which we must return very soon in an attempt to correlate the results obtained. Meanwhile there remain two series which were to provide further opportunities to verify an hypothesis which had begun to suggest itself at this time.

Series G. If—as our earlier results indicate—anything like inherence is responsible for the effects we have obtained, it should be possible to reduce the stability of the phenomenon by decreasing the stability of the perception involved. All of the designs employed thus far had been highly conventional in nature, in which one gray fell indisputably outside the usual boundaries of some recognizable design. If figures of a non-conventional sort were to be exposed, our earlier results would lead us to expect somewhat less frequent reports of the included gray as lighter. Ten designs of this sort were constructed, none of them being recognizable as a familiar object. (One such design is shown in Fig. 1, d.) Each observer made twenty reports upon each design with results as shown in Table VIII.

Again the conditions predicted are fulfilled, for there is a marked drop in the frequency with which the included gray is reported as brighter, and an equally marked rise in the reports of the excluded gray as brighter. Lo shows an actual reversal

TABLE VIII

Obs.	Included Gray (G_i) Brighter	Excluded Gray (G_o) Brighter	Same and No-Report
Fe	60.0%	35.0%	5.0%
Fr	59.5%	30.5%	10.0%
Kr	77.0%	5.5%	17.5%
Lo	27.5%	48.0%	24.5%

of the direction of her choice, reporting the excluded gray as brighter more often than the included gray. The other observers show marked drops from their ratios with the conventional designs. Again there is a demonstration that perceptual factors of some sort are effective in these cases of visual contrast; and there is a suggestion that one of these factors may be the '*Zugehörigkeit*' advanced by Benary.

Series H. The results assembled thus far, as well as certain common trends in the descriptive reports, suggest that the patches receive unequal induction-effects from the figure and the background. We may apply a first test to this theory by investigating Benary's statement that the differences between the two grays appeared when sheer outline designs were employed. Here, with background and figure made of the same white, we should expect but small and instable differences to appear, providing that our assumption above is correct. Ten outline figures (like Fig. 1, e) were constructed and each observer made twenty reports upon each design with the following results.

TABLE IX

Obs.	Included Gray (G_i) Brighter	Excluded Gray (G_o) Brighter	Same and No-Report
Fe	27.0%	20.5%	52.5%
Fr	59.5%	20.0%	20.5%
Kr	21.0%	13.5%	65.5%
Lo	35.5%	32.5%	32.5%

With the single exception of Fr, it is apparent that we are dealing with reports which show practically a chance distribu-

tion, with a high percentage of cases in the uncertain column. Even in Fr's case, the reduction of the ratio from that of the high-inherence designs is marked. Reference to the reports given by the observers shows that all of them took the designs as black outlines upon white surfaces, rather than as white figures upon a white background. This may have aided in producing a chance distribution; and there is, at any rate, a strong suggestion that the two grays receive unequal induction from their figures and their backgrounds, which accounts, at least in part, for the differences obtained with the earlier designs.

Interpretations and Conclusions

The most obvious contribution of the experimental work just reported is its vindication of Benary's statement that the difference between the two grays cannot be explained entirely in terms of peripheral factors. In most of the high-inherence designs, the included gray was reported as brighter in practically every exposure; and in all the rest of these designs, the average obtained was so high as to leave no doubt regarding the existence of a consistent brightness-difference between the two patches. The reduction of these values, through attempts to reduce the inherence of the included patch, suggests that some factor analogous to 'inherence' is responsible, a suggestion which receives further support from the results with the non-conventional figures. As a first step towards a tentative explanation of our results, it looks as if we should be compelled to admit the efficacy of central factors in these cases at least. Disregarding for the moment any purely 'psychological' interpretation, such as that offered by Helmholtz, we may point out that the conclusion just stated is by no means novel. As stated in giving the history of this problem, such a conclusion is in accord with various experimental studies of fairly recent date, many of which have been reported by men researching under the influence of *Gestalttheorie*. Thus Fuchs [9] found that perceptual factors may overshadow the 'normal' contrast effect; Rubin [10] has reported

[9] W. Fuchs, Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten, *Zsch. f. Psychol.*, 1923, 92, 298.

[10] E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 6.

experiments of the same general purport; and Jaensch [11] is so influenced by his experimental work as to propose a 'parallel-law' for the phenomena of contrast and memory color. A brief summary of these studies is provided by Koffka.[12]

We are compelled to take up a position on this side of the controversy and to agree that central factors are effective in the case of designs similar to those employed in these studies. So far, then, we may go with Benary and the others; but a careful study of our results offers convincing evidence that Benary was not justified in abandoning his explanation with the statement that the included patch is *zugehörig* with the figure. Our observers frequently report that this patch is taken as part of the figure, indeed, as belonging to it; but to allow the description to rest here is to ignore much valuable descriptive material, and to give, as we shall show, an entirely erroneous impression of the matter. Descriptions by our observers show definitely that the difference between the two patches is far more than a mere difference in inherence. We have quoted a list of the major differentia, and now we have to discuss two meanings of the term 'prominence' which appear in the reports on Series F. There, as we found, the excluded gray was described as being more prominent in two different ways. In the first place, it was prominent spatially: it 'protruded,' 'stuck out,' was 'a projection from the figure,' or was 'something added to the figure.' It was incongruous in the position in which it appeared; the included gray, on the other hand, fitted in well and was taken as belonging in the position in which it was seen.

The second use of the term may best be introduced by a series of quotations from the reports given by the various observers.

An: "This (excluded gray) is simply the one I look at. The other is, to a certain extent, disregarded."

Fe: (The excluded patch) "possesses something that I should characterize as a qualitative dominance, outstandingness, prominence, insistence."

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[11] E. R. Jaensch, Parallelgesetz über das Verhalten der Reizschwellen bei Kontrast und Transformation, *Zsch. f. Psychol.*, 1920, 83, 342.

[12] K. Koffka, Perception; an introduction to Gestalt-Theorie, *Psychol. Bull.*, 1922, 19, 531.

Fr: "It (excluded gray) stands out to meet the eye at once, while the other has to be sought."

Gl: "The (excluded) gray caught my attention first, and I had to look rather carefully to discover just exactly where the (included) gray was."

Ja: "In such cases as this, the part of the figure which has this out-of-placeness is central, just as a particular word one is reading is central. The other (included) gray is marginal, just as the other words on that page are."

Ke: "The (excluded) gray caught my attention at the beginning, and held it."

Kr: "The (excluded) patch was apprehended first; then the (included) one emerged."

Lo: "It seemed as if there were a light . . . concentrated at that (excluded patch) so that the (included patch) was obscure."

In these typical reports it is obvious that the observers are describing a difference in perceptive value of the two patches, a description which is second in frequency only to references to something like the 'inherence' of the patches. The excluded patch, whether or not the BkW or the WBk form was employed, is described as being more 'attention-compelling' than the included patch. As the situation stands, then, we have a demonstration that the two patches differ in what has commonly been called 'attention-value;' and an adequate explanation of this is the fact that the excluded patch is described as incongruous and out of place, both of which states are alleged conditions of attention. But we must go further at once and note that this is no simple difference in attributive clearness, as further references to the reports of the observers will show. In fact, these reports present a strong suggestion that we are dealing with something remarkably like the figure-ground relationship described originally by Rubin.[13] This investigator has compiled a list of characteristics which constitute what he calls the *Figurcharakter*, and Koffka [14] has advanced further experimental evidence in support of this list. When one of our observers spontaneously described the excluded gray as being '*Figur* in the *Gestalt* sense,' it seemed worth while to compare the descriptions given by our observers with the items on Rubin's list. Once this was done, a striking parallel between the two lists became apparent, especially as the observers had received no more

[13] E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 35.

[14] K. Koffka, Perception; an introduction to Gestalt-Theorie, *Psychol. Bull.*, 1922, 19, 553.

specific instruction than to report upon what they meant by 'prominence.'

To sum up the situation, we may remark that there is a close resemblance between the characteristics of *Figur* and the descriptions of the excluded patch. Thus Rubin describes the *Figur* in terms synonymous with the following delineations of the excluded patch; it is 'more definite in contour,' 'better localized,' 'compact,' 'surfacy,' and 'more definite.' The included patch, on the other hand, is described by our observers as 'filmy,' 'weak,' 'indistinct,' as 'a hole in the figure covered over by a mist,' and perhaps not even observed; and these, we find, are the attributes assigned by Rubin to groundal structures. We find that several of our observers describe the excluded gray as being *there* at the beginning of the exposure, while the included gray which has to be sought emerges later. This is strongly reminiscent of the phraseology employed by Gelb and Granit [15] when they state that coloration on the figure is 'clearly there all at once,' while coloration upon the ground is 'not so suddenly visible.'

When we say that the included gray is apparently groundal in nature, while the excluded gray tends to be figural, we have cast our lot with those who insist that the older analytic psychologies are unable to give an adequate account of such phenomena in terms of attensity of individual process. Reference to the reports of our observers compels us to abandon the attempt to explain the difference between the two grays in terms of clearness-degree alone; but there is good reason to hesitate before accepting outright the *Figur-Grund* explanation in its present setting. In the first place, although Rubin seems to have realized that this relationship is no simple matter of two distinct levels, many later writers in *Gestalttheorie* seem to assume that this is the case, and that whatever fails to fall into the rubric of *Figur* must be relegated definitely to the groundal level. Our observers, on the other hand, report frequently in terms of several levels of organization, and their descriptions indicate that the black object ordinarily

[15] A. Gelb & R. Granit, Die Bedeutung von 'Figur' und 'Grund' für die Farbenschwelle, *Zsch. f. Psychol.*, 1923, 93, 83.

occupies quite a different level from that of the excluded patch, with the included patch and white background on still different perceptual levels. Explanation of these reports in terms of a simple figure-and-ground dichotomy would be a difficult task indeed.

A far more serious objection must be levelled against the tacit assumption that the experiential *Figur* is coordinate with the major physical object in the stimulus-field. The evidence contained in our reports is directly in opposition to such an interpretation, and further contrary evidence is available in Benary's own work as well as in the study of Mikesell and Bentley. Frequent references from our own observers show that the black objects are not taken as figural in the same sense as the excluded patch, and not infrequently we find that part of the black object is taken as figural and part more nearly as groundal. With this in view, it is not surprising to find that Benary reports (p. 138) certain areas of the black designs as being critical for changes in the brightness of the patches; outside of these 'critical regions' large areas of the design may be covered without affecting the brightness of the patches, while small changes within these 'critical regions' produce marked shifts in brightness. Further support for the theory that merely part of the figure may be focal is to be found in the series in which Mikesell and Bentley (Series iv) found relatively little change in brightness whether the grays were taken as parts of the design or 'in abstraction.'

Benary's assumption that the cross, for example, is the *Figur* is likely to lead one into serious logical difficulties. Thus, if the design is figural and the included gray is taken as part of the design, one might readily conclude that this patch should be figural. We cannot emphasize too strongly the fact that this statement is diametrically opposed to the conditions existing in these patterns, for the experimental evidence available designates the excluded gray as figural, while the included gray tends to be groundal. We are faced with a dilemma in which we find the explanation in terms of attensity inadequate, and the *Figur-Grund* conception inacceptable for logical reasons. The way of escape must be provided by

some explanation which accepts the experimental background of the latter interpretation but avoids its logical difficulties. Tentatively we may accept some such position as that offered by Bentley [16] when he says that "attention appears . . . in the mental complex . . . as a close or loose organization of clear and obscure members integrated . . . under the . . . conjoint influence of stimulus, receptor and central trend." Figure, under such an interpretation, becomes a focal and closely-knit organization of experience, with all possible degrees of lower organization between it and the ultimate form of groundal organization. We may thus speak of a unit as 'more focal' or 'more groundal' than another, and in this way provide an adequate description of the various levels reported by our observers.

The way to a tentative explanation of the phenomenon is now clear. The incongruity and out-of-placeness of the excluded patch (its low inherence to the main geometric design, if you please) go to make the observers perceive it in a type of organization which tends to be figural and highly organized. Turning to the literature, we find that figure is more resistant to changes in color and brightness than ground.[17] As one of our patches tends to be figural and the other groundal, it is only to be expected that the two will appear to be different in brightness, even if subjected otherwise to identical contrast-induction (a situation which we shall presently suggest does not exist.) Thus, if the situation is such as to make the patches appear lighter, the excluded gray, being figural, should resist this change and appear darker (and this is the status of the grays when black figures on light backgrounds are exposed). If, on the other hand, conditions are such as to make the grays appear darker, we should expect to find the figural excluded gray lighter in appearance (as actually occurs when white designs on black backgrounds are employed). If we make the reasonable assumption that the more figural parts of a design exert greater contrastive influence upon surrounding areas than do the more groundal parts, the above explanations appear to be satisfactory in view of the evidence available.

[16] M. Bentley, *The field of psychology*, 1927, 177.

[17] E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 35 ff.

There is reason to believe, however, that conditions are not so simple as those just described, and that the two grays are exposed to unequal contrast-induction. Thus Benary remarks that naive observers are not at all surprised by the difference in brightness between the grays, as the included gray is seen as 'on black' (in the BkW designs) while the excluded gray appears to be 'on white.' Our observers give frequent reports of this nature; and the work upon memory color and similar phenomena makes it appear reasonable that the included gray is accordingly affected to a greater extent by the color (or brightness) of the figure, while the excluded patch is more affected by the background. This, of course, would increase the tendency for them to appear as we have noted above. The obvious test of such a conclusion is to work with colored designs, in the hope of obtaining final evidence by means of a different color-induction in the two patches. This has been done, but the results thus far are, unfortunately, inconclusive. Benary reports that his observers, when shown a blue design on a yellow background, reported the excluded gray as bluish and the included patch as yellowish; this result is in complete agreement with the hypothesis which we have just advanced. Our own attempts to use colored designs met with indifferent success, and there was a tendency to report both grays as tinged with the same color. With exposure times of 1000 sigma, and an attempt to adjust the stimulus-colors for variations in saturation, there was still much variation between observers, although the results in general were favorable to Benary's conclusion. The induced hues are so weak that there is serious difficulty in naming them; in fact, Brown and Hoisington [18] have shown that weak colors are named 'wrongly' more often than 'correctly.' The question must wait until a method is devised for obtaining greater saturation of the induced color. There is, however, nothing in these results contrary to the working hypothesis which we have just advanced.

[18] R. G. Brown & L. B. Hoisington, The dependence of hue on difference in tint, *J. Gen. Psychol.*, 1928, 1, 108.

Summary

Experiment has shown that changes in the inherence of the gray patches in these designs result in changes in their relative brightness. The excluded gray, being less 'inherent' (or more incongruous) tends to be apprehended in a higher type of attentional-perceptual organization than the included patch. As this figural type of organization has been shown to be more resistant to changes in brightness, we might expect that the excluded patch would resist such changes more than the included patch. If we add to this the fact that the excluded gray appears 'against the background' and the included gray 'against the figure,' we have a tentative explanation of why two grays with identical contrastive borders should appear persistently to differ in brightness. A more important accomplishment of this investigation, however, is its demonstration that central factors must be considered in these cases of visual contrast, if not in all such cases.

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ATTITUDES AND THE GALVANIC SKIN REFLEX

BY THEODORA MEAD ABEL[1]

Sarah Lawrence College

We shall speak here of 'attitude' in the sense of the *Bewusstseinslage* of Mayer and Orth[2] and of the long list of researchers who were bent upon the discovery of the elements of thinking (*Gedankenelemente*). When so used the word means a very brief and flashing but highly significant experience which seems to come all-of-a-piece and to be not readily resolvable into the ordinary constituents of sensation and image. With the history[3] of these studies we are not here concerned, but only with a functional relation of certain of the attitudes to those bodily processes in and beneath the skin which are registered as electrical changes and which are widely spoken of as the 'galvanic skin reflex' or, in a much cruder way, as the 'psychogalvanic reflex.' With the nature and explanation of these bodily changes we shall not deal. The literature of the subject is already very extensive; but it has been well summarized and reviewed.[4]

Most studies of the attitudes have been directed toward their description and their general function in understanding

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[2] A. Mayer & J. Orth, Zur qualitativen Untersuchung der Association, *Zsch. f. Psychol.*, 1901, 26, 1–13, and J. Orth, Gefühl und Bewusstseinslage, *Samml. u. Abh. a. d. Gebiete d. Päd., Psychol., u. Physiol.*, 1903, 6, 223–253.

[3] The early works are reviewed and criticized in E. B. Titchener, Lectures on the experimental psychology of the thought processes, N. Y,. 1909. Later references are H. M. Clarke, Conscious attitudes, *Amer. J. Psychol.*, 1911, 22, 214–249; R. Hönigswald, Die Grundlagen der Denkpsychologie, Leipzig, 1925, and M. Bentley, The field of psychology, N. Y., 1927, 351–360.

[4] Useful reviews and bibliographies are to be found in *Psychol. Bull.*, 1929, 26, 64–119, 185–201.

and thinking. Most studies of the electrical changes in the skin have been directed toward physical measurement and record, toward the underlying bodily conditions, and toward their alleged connection with emotions and other 'mental' phenomena. We have related these two matters in our experiments because (1) it appears that at least certain of the attitudes or *postures* (Bentley) stand as indicators of a comprehensive state of the organism in which, presumably, the autonomic and secretory functions are involved and (2) it is obvious that these experiences are very much simpler and briefer than the complicated and changing emotions, conations, and the like, which many investigators have tried with indifferent success to correlate with electrical changes in the skin. In the descriptive literature of the attitudes only casual, or at least uncontrolled, reference has been made to bodily factors;[5] on the other side, physicists and physiologists have made precise measurements of changes in bodily potential but have generally dealt in a gross and uncontrolled way with the alleged 'mental' correlates of these changes.

We have therefore conceived the possibility of pursuing further the physiological way of approach and of finding out whether the galvanic skin reflex accompanies such attitudes as doubt, surprise, hesitation and questioning, which commonly occur during the course of comprehension and elaborative thinking.[6]

[5] One experimenter (J. T. Metcalf, An experimental study of the conscious attitudes of certainty and uncertainty, *Psychol. Monog.*, 1917, 23, (no. 100), 181–240) has tried to relate the attitudes of certainty and uncertainty to the accuracy, rate and intensity of drawing movements of the hand. Here *attitude*, taken in the strict sense, was scarcely vouched for in the observers' reports and the bodily correlation was extremely limited. The writer "hesitates to claim anything final for his results" (240).

[6] The topic of the developmental or genetic aspect of the postures deserves consideration. There seems to be a consensus of opinion that the postures are remnants, the mere shadows of more complex phenomena earlier experienced by the individual or the race. W. F. Book (The psychology of skill with special reference to its acquisition in typewriting, *Univ. Montana Publ. Psychol.*, 1908, 1) and K. Koffka (Zur Analyse der Vorstellungen und ihrer Gesetze; eine experimentelle Untersuchung, Leipzig, 1912) treated them as of imaginal origin shrunk through repetition to a mere shell of their former selves. For Titchener (*op. cit.*) and Washburn (Movement and mental imagery, Boston, 1916) they are fragments of motor attitudes. Bentley (The field of psychology, N. Y., 1927) regards them as transfers from organic states into posture-meanings, "announcing from moment to moment the position of the organism upon matters at

It has for some time been obvious that the changes of potential and of resistance commonly called the galvanic skin reflex (GSR) are not equivocally related to *any single* psychological fact or state, and very likely not indicative of *any single* state or condition of the central nervous system.[7] So far as emotion is concerned, the electrical method of indication seems to have gone the way of the other 'methods of expression.' We must not then deceive ourselves with the vain hope of finding in the attitudes the essential and unique correlate of a physiological condition of the body. At the same time, we have thought it worth while to ascertain whether these momentary turns and trends do not possess a physiological indicator sufficiently simple and sufficiently constant to yield a significant correlation. Again, our method of commentative report under experimental control has promised to yield new information upon the attitudes themselves.[8]

The Experiments

Apparatus. Our galvanometer was of the d'Arsonval moving-coil mirror type.[9] We used a universal shunt, a

issue" (356–357), and he assigns an important function to them in thinking. The postures seem to be survivals that have been of vital concern to the psychological organism in making its adjustments during the progress of thought. It appears that the organism may become widely involved, muscularly, organically or affectively, in their production.

[7] GSR has been recorded in sleep, general anaesthesia, and under operative conditions in various animals. The mere excitation of receptors seems also, under certain circumstances, to induce changes of electrical resistance.

[8] Several observers recording the GSR have more or less incidentally used in their observers' reports the following terms suggestive of attitudes; astonishment, surprise, satisfaction, relief, relaxation, consternation, bewilderment, confusion, strain, excitement, annoyance and amusement. These investigators were so intent upon translating the observers' protocols into the categories of conation, emotion and volition that they overlooked the possible significance, in terms of the organism, of the postural comments themselves regardless of their arbitrary categorical setting. Cf. R. C. McCarthy, The measurement of conation, 1926; H. M. Wells, The phenomenology of acts of choice; an analysis of volitional consciousness, *Brit. J. Psychol. Monog. Suppl.*, 1927, 4, no. 11; B. P. Stevanovic, An experimental study of the mental processes involved in judgment, *ibid.*, 1927, 4, no. 12.

[9] Leeds and Northrup, no. 16294.

Wheatstone bridge and liquid electrodes.[10] Eight dry cells supplied the external current.

In order to be able to compare deflections in terms of distance on a millimetric scale, for the various Os and for the same O at different times, regardless of the magnitude of the O's initial resistance, we insured a constant current through the galvanometer by means of a slide-wire resistance coil (1400 ohms). Before putting an observer in series with the Wheatstone bridge, we connected him in a simple circuit with the external current and the galvanometer with the shunt set at .001. By means of the resistance coil we brought about a given deflection on the scale (30 mm in our first series; 40 mm in our next three series).[11] When this initial intensity had been assured, we shifted the observer over to the Wheatstone bridge by means of two double-throw switches. We then balanced the bridge, with the shunt at 0.1 sensitivity, and were ready to proceed with the experiment.

This procedure was carried out each time before initiating the problem.

At the beginning of each experimental hour we waited at least 10 min. after the observer had been placed in the electric circuit in order to eliminate the initial deflection called by Veraguth the 'curve of rest.'[12]

For recording deflections we used an encased single-filament lamp with a narrow slit for releasing the light. The lamp was suspended at a distance of one meter from the galvanometer. The beam of light was deflected from the mirror of the galvanometer to another mirror just under the lamp and placed at such an angle that the beam was then projected on a spot upon the table. Just in front of the mirror of the galvanometer we placed a small plano-convex lens with a focal distance of one meter which decreased the diffusion of light to such a degree that when the beam was projected on the table it was a narrow strip 2–3 mm in width. Other sources of light were screened away from the table. The movements of this beam of light were traced by the experimenter with a pencil writing on a continuous wide strip of paper moving slowly across the table. The paper unrolled under the table at the back, came up over a roller on the far side, and down over another roller on the near side of the table where it was wound up by a simple kymograph on a wooden roller. This method avoided the noise and complications involved in gearing down an electric motor.

The paper, which was 36 cm in width, passed freely over glass and under a steel scale graduated in mm. When no current was going through the galvanometer, the beam of light fell at zero on the scale. The experimenter traced the deflections along the edge of the scale, which both enabled him to read (if he so desired) the size of the deflections at the time and held the pencil steady.

An electro-magnetic time-marker and a seconds pendulum recorded the time in pencil at one edge of the paper.

The apparatus was cut off entirely from the field of vision of the observer by means of screens. O sat in a chair with an

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[10] We employed a Leeds and Northrup Wheatstone Bridge, Post-office type, with plugs, a General Radio Co. shunt, type 229, res. 2.23 ohms. The differentials of the liquid electrodes were Nacl and Zn_2SO_4.

[11] Knowing the distance of the scale from the galvanometer (115 cm), the sensitivity of the galvanometer, the resistance of the shunt, etc., we calculated the amperage for 40 mm at .0000516 amp.

[12] This seems to be a polarization effect. Cf. O. Veraguth, Das psychogalvanische Reflex-Phänomen, Berlin, 1909.

adjustable head-rest. His right arm rested on a felt-covered pad. The distal end of the middle and index fingers was immersed in the saline solution of the electrodes, which were fitted into holes in the arm of the chair. These fingers were washed with benzine before each sitting. O faced a screen where the stimuli were exposed behind a shutter.

Problems for the arousal of attitudes. We tried to devise problems that met the following requirements:

i. Variation from easy to difficult for the various Os, thus insuring the arousal of varied attitudes as well as both deflecting and non-deflecting conditions
ii. Brief exposure to reduce the difficulties of report
iii. Simplicity of context to shorten O's comprehension of the problem
iv. Uniformity of solution as 'right' or 'wrong' to avoid the necessity of search for a formulated verbal reply.

The problems of our main series which seemed to meet the above requirements were mainly of three types:

a. Proportions

E.g. $\frac{\text{Fuel}}{\text{Combustion}} :: \frac{\text{Stimulus}}{\text{Nervous energy}}$

E.g. $\frac{\text{Volcano}}{\text{Igneous}} :: \frac{\text{Earthquake}}{\text{Granite}}$

E.g. 4 geometrical figures drawn to represent a kind of proportionality

b. Statements of fact

E.g. Indo-European is part of Aryan
E.g. Oxygen is heavier than ozone
E.g. Plantar reflex is part of a dog

c. Configurative schemes

E.g. Outlined profiles placed right and left on the exposure card, one marked 'more' and the other 'less' beautiful
E.g. Two chemical expressions (H_2S and H_2SO_4) one marked 'more' and the other 'less' volatile

For our materials we ranged over the fields of physics, biology, history, geography, and general knowledge.

The problems were printed or drawn in India ink on white cardboard 14 cm x 8.3 cm. In the main series were 95 problems (20 in Series C, 25 in Series D, 25 in Series E, 25 in Series F). The series did not differ as to type of problem except that in Series C they were slightly easier. The observers were given a preliminary series of 15 to 20 problems in

order to accustom them to the situation and to prevent deflections occurring from general excitement due to a novel setting.

Observers. The work was carried on in the Psychological Laboratory of the University of Illinois in two periods, during the autumns of 1927 and 1928. Series C (20 problems) was given to six Os (Culler, Cu; Moller, Mo; Sullivan, Su; Fox, Fx; Jenkins, Jk; and Glanville, Gl), in the first experimental period. Cu, Mo, Su, Fx, Jk were on the teaching staff in Psychology. Gl was a graduate student. During the second year, Cu, Mo, Su, Fx worked on the next three series D, E, F, (75 problems) after a second preliminary series. A new observer (Hatfield, Ht), a member of the teaching staff, was given only Series D and E (50 problems).

Procedure. The observers worked two hours a week, solving three or four problems in the experimental hour. The time of day and other fixed conditions of experimentation were generally kept constant for each O.

After being placed comfortably in the chair the following instructions were read to the observer.

"You will first be asked to sit quietly for a few moments."
(During this period E starts the apparatus, insures a constant current through the galvanometer and balances the Wheatstone bridge; then starts the kymograph and the time marker.)
Next signal: "Just a moment longer."
(This is the beginning of the *fore-period*: deflections, if any, are recorded.)
"At the signal 'ready' a card will appear in the screen before you."
(The 'ready' is the beginning of the *exposure-period.* E lifts the shutter by means of a pulley arrangement, thus exposing the card.)
"Regard attentively the material exposed. Consider whether this material is correct or incorrect. After the exposure sit quietly without answer."
(When E drops the shutter the *after-period* begins.)
"Later the signal 'report' will be given."
(End of *after-period* and of tracing deflections. The kymograph and time marker are stopped.)
"In reporting say 'correct' or 'incorrect,' and then give as full and as accurate a commentative report as you can for the entire exposure-time. Try as far as possible to report in terms of your functioning organism rather than in terms of the object out there. If you have anything in particular to say about the period just before the card was exposed or about the period after the card disappeared, you may add that; but the chief reporting should be confined to the exposure-time. Be sure to make your report as full and as accurate as possible."

In Series C the lengths of the periods were

Fore-period 10 sec
Exposure-period 5 sec
After-period 10 sec.

In Series D, E and F the time was changed to F = 10 sec, E = 10 sec, A = 20 sec, as being more feasible. The experimenter kept time by means of a stop-watch that was suspended in his direct line of vision.

Results

When the experiments were carried out as we have described, O's attempt to solve, or at least to comprehend, the problem presented was frequently, but not always, accompanied by a deflection of the galvanometer. The deflection was carefully followed and traced by the experimenter.[13] The relation of problems to deflections is set down in Table I.

Table I

Obs	No. of solutions	No. of Deflections			Total
		Fore-period	Exposure-period	After-period	
Su	95	23	33	12	68
Fx	95	17	58	15	90
Cu	95	10	17	16	43
Mo	95	26	31	21	78
Ht	50	4	8	4	16
Jk	20	3	14	2	19
Gl	20	5	17	1	20

It is apparent that individuals differed in the total number of deflections; Cu and Ht showing (relatively) the least, and Fx, Jk, and Gl the greatest number.

Among all Os the highest frequency of deflection occurred during the exposure-period; although this duration was only half that of the fore- or the after-period.[14] If practice and adaptation to the experimental setting tended to decrease the number of deflections (see Table II), the effect was well masked by the character of the problems or other factors. Ser. C gave fewer deflections (39) than any of the three succeeding series (84, 54 and 71).

[13] Deflections presumably due to bodily movements of O, to a chance cough, and the like, were thrown out of the records.

[14] Here and in subsequent tables, *Fore-period* begins with "Just a moment longer" (when the bridge has been balanced) and ends with "ready": *Main-period* begins at "Ready" and ends with the drop of the exposure-shutter: *After-period* begins here and ends with "report."

TABLE II

Obs	Period	Frequency of deflection in the four series C	D	E	F	Total
Su.....	Fore	6	7	2	5	20
	Main	6	12	6	9	33
	After	0	4	2	5	11
Fx.....	Fore	1	8	3	5	17
	Main	16	19	11	12	58
	After	1	4	5	5	15
Mo.....	Fore	5	7	4	10	26
	Main	4	7	9	11	31
	After	0	5	7	9	21
Ht.....	Fore	0	2	2	0	4
	Main	0	6	2	0	8
	After	0	3	1	0	4
Totals		39	84	54	71	248

Under our conditions, the latent time of GSR was usually 2–3 sec.[15] Table III represents the frequency and the time of onset of the deflection.

TABLE III

Obs.	Period	Time of onset of deflection (sec) 3.5 sec or less	4–7.5 sec	8 sec or more	Total
Cu.....	Fore	4	4	1	9
	Main	5	9	1	15
	After	0	0	0	0
Fx.....	Fore	5	9	3	17
	Main	25	18	4	47
	After	6	9	2	17
Su.....	Fore	4	5	3	12
	Main	15	12	1	28
	After	5	6	0	11
Mo.....	Fore	7	13	4	24
	Main	14	14	1	29
	After	3	10	4	17
Ht(D & E).....	Fore	0	2	1	3
	Main	1	6	0	7
	After	1	3	0	4
Jk(C).....	Fore	0	2	3	5
	Main	4	5	4	13
	After	8	0	12	20
Gl(C).....	Fore	2	1	1	4
	Main	6	6	3	15
	After	1	0	1	2
Totals.....		116	134	49	299

[15] Lacking photographic records, the onset of the deflection could not be shown exactly; but the reaction time of E in following the deflections did not exceed 0.5 sec, with a relatively small mv. The first signal ('sit quietly') was followed by a few deflections, but by no more than came later in the fore-period.

In the main (exposure) period just as many deflections (70) appeared during the first 3.5 sec (after the 'ready' signal) as in the next fraction, 4–7.5 sec, of this period. The deflections therefore coincided with the perceptual apprehension as well as with the subsequent operations of O, *i.e.* realization of difficulty, grasping the significance, and the solution.

TABLE IV

MAXIMAL EXTENT OF DEFLECTIONS IN SERIES D, E AND F[16]
(Shunt set at 0.1)

Obs.	Ave (mm)	$P.E._A$.	No. of cases	Range (mm)	Excluded cases (deflections beyond scale)
Cu	24.48	13.43	31	2–89	1
Mo	55.71	12.75	53	3–180	9
Su	44.34	23.17	40	2–143	5
Fx	54.03	28.85	49	2–180	9
Ht	29.29	14.35	14	7–72	0

Table IV summaries Series D, E and F. In no instance is there a significant central tendency in the magnitude of the deflection. The P. E. s are relatively large. Two observers, Cu and Ht, had considerably lower averages than did the other three. These Os also had the smallest (relative) number of deflections.

The commentative reports of our Os were full of attitudes, which fell into two characteristic groups, those suggestive of difficulty, check or upset, and those signifying smooth and easy activity. The observers had three methods of indicating them; (1) by a direct designation of surprise, amusement, hesitation, doubt, relief, satisfaction, etc; (2) by a verbal comment or invective while working out the problem, *e.g.* "my God," "how am I expected to know?," "Gee whiz, there's a snag somewhere," or "easy," "a cinch," and (3) the observers reported attitudes as having occurred without verbal accompaniment as "a flashlike wave of something that would have meant 'I'm not sure" or "some kind of surge that meant questioning."

The reports frequently contained a description of such bodily feels as "a dense unpleasant pressure in the abdomen,"

[16] The initial Series C is here omitted because, at the beginning, the shunt was not always maintained exactly at 0.1.

or "a warm glow of pleasure . . . bright pressure that spread through the organism." Somæsthetic experience often accompanied the attitudes. There were very few instances of reports on pleasantness and unpleasantness, for which we had not specifically called.[17]

The reports were classified in the following manner. *Group PR* (predicamentive reports) included reports in which were one or more clear and strong attitudes of the 'difficulty-met' or 'predicament-encountered' type (*e.g.* 'surprise,' 'gee whiz,' 'got stuck,' 'searched around') and indications of somæsthetic experiences involved in a predicamentive situation, as 'reacted with my whole organism, upset,' or 'heavy dull pressure in my chest which meant that I was stuck.' *Group* PR_L (reports of light predicament) consisted of the predicamentive comments, and postural or organic feels which were characterized as 'light and weak,' as 'a little upset,' 'a tiny glow of pleasure,' or 'just a faint tinge of doubt.' *Group ER* (easy report) meant no difficulty or predicament. The comments showed that the situation had been met by a smooth and progressive working out; *e.g.* 'nothing to it,' 'saw it right away,' or 'worked hard for a moment but got it without difficulty.' 'Easy reports' did not always imply that the problem was solved at a glance; at times O had to 'search around,' to 'consider several possibilities;' but no check was offered to the smooth progress of solution.

Examples of complete reports of the three kinds, PR, PR_L and ER are as follows;

PR: "Very much stirred up by first appearance of the figures. The pattern was surprising, novel. Saw 'symmetrical,' but didn't know what it meant at first. Then saw 'more' and 'less' at bottom. Looked from one figure to the other. First made a judgment that figure at left was more symmetrical. Then saw extra curve at left. It set me thinking of the right as more symmetrical. Kept arguing with myself, and finally decided one at left was more symmetrical. Was completely sure just as exposure ended." (O = Fx: Ser. D_4.)

PR_L: "Read the problem in verbal-motor terms. Then a moment of slight consternation when I realized that I didn't know. Then I just gave up. I didn't get excited about it as there was nothing I could do, so I remained calm." (O = Su: Ser. E_8.)

[17] In preliminary experiments at Cornell we had used the descriptive terms, pleasant and unpleasant, bright and dull pressure; but we found that these experiences took place indiscriminately and independently of any deflection.

ER : "Visual experience translated into kinaesthetic terms. Then 'I had something like this before' (throat kinaesthesis). Then I described it and answer came 'no' a little before drop went down." (O = Mo: Ser. D_{24}.)

Table V shows the number of reports falling under PR, PR_L and ER. The various Os differed in the frequency of a given type of report. Su, Fx, Jk and Gl gave most PRs: Mo and Ht had more easy than predicamentive reports: Cu more PR_L reports.

TABLE V

FREQUENCY OF PR, PR_L AND ER REPORTS

Obs	PR	PR_L	ER
Cu	20	47	26
Su	45	10	36
Fx	57	14	12
Mo	18	12	57
Ht(D & E)	12	7	26
Jk (C)	16	0	4
Gl (C)	16	0	4
Totals	184	90	165

When we compared the three types of commentative report with presence or absence of GSR, we found the following correlations:

(1) D:PR A deflection occurred with a predicamentive report.
(2) D:PR_L Deflection and slight predicament.
(3) D:ER Deflection and 'easy' report.
(4) ND:ER No deflection and 'easy' report.
(5) ND:PR No deflection but a predicament.
(6) ND:PR_L No deflection and slight predicament.
(7) DX:PR Time of the deflection and time of the commentative report show no coincidence.

TABLE VI

FREQUENCY OF DEFLECTING AND NON-DEFLECTING CONDITIONS WITH REPORTS OF PREDICAMENT, SLIGHT PREDICAMENT, AND EASE DURING EXPOSURE-PERIOD

Obs.	Cu		Fx		Su		Mo		Ht		Jk		Gl		Total
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	no.
D:PR	9	9	44	46	27	28	17	17	6	12	10	50	13	65	126
D:PR_L	5	5	0	0	1	1	1	1	1	2	1	5	0	0	9
D:ER	2	2	7	7	3	3	9	9	1	2	3	15	3	15	28
ND:ER	24	25	15	16	33	35	46	47	25	53	1	5	1	5	145
ND:PR	6	6	9	9	14	15	0	0	5	10	3	25	2	10	39
ND:PR_L	47	49	14	15	10	11	12	12	7	15	0	0	0	0	90
DX:PR	1	1	4	4	4	4	3	3	1	2	0	0	1	5	14
															451

	Deflection	No deflection	Approx. ratio
PR	126	39	3:1
PR_L	9	90	1:10
ER	28	145	1:5

Table VI presents the number and percentage frequencies of these various correlations. The general outcome is that deflections upon the galvanometer are frequently associated (ratio 3:1) with clear and decided attitudes of doubt, difficulty, puzzlement and hesitation, as attested by the commentative reports, and that the absence of deflection is frequently associated with no reportable attitude of the same general type (ratio 5:1). Slight but reportable checks to the course of understanding and elaboration are usually associated with no galvanometric deflection (ratio 10:1). On the other hand, so many attitudes reportable in the verbal phrases 'I see,' 'This is going well,' 'Easy,' 'No difficulty here,' and the like, appear without deflection that we cannot infer that attitudes at large are indicated by a change in electrical resistance but only that *certain kinds* of attitude are so indicated. It looks, then, as if the organism, when it operates by way of comprehension and of elaborative solution in such tasks as we have set our observers, initiates certain phases of autonomic function that are modified when the operation is suddenly checked. If we still wish to speak, in this connection, in the vague terms of 'emotion,' it is obvious that we must refine our terminology.[18] As for the neural side of the matter, it is easy to invoke 'inhibition'; but possibly it is as well to omit that convenient but overworked term.

As regards those negative instances where deflections appear in the absence of the checking sort of attitude, we make four comments. (1) Since we know that the electrical resistance of the skin is changed by non-attitudinal conditions, it seems reasonable to suppose that certain of these imperfectly known conditions were here operative. It is possible that when these conditions are both recognized and removed, some of the negative cases (in the above sense) may disappear. New experiments with psychological and physiological refinements are here needed. (2) Some of our predicamentive states were plainly verbal; *i.e.* the observer noted 'surprise'

[18] Relevant references are R. J. Bartlett, Does the psychogalvanic reflex phenomenon indicate emotion? *Brit. J. Psychol.*, 1927, 18, 30–50, and N. Bayley, A study of fear by means of the psychogalvanic technique, *Psychol. Monog.*, 1928, 38, (no. 176), 1–38. Bayley's 'Startle' is certainly not our attitude.

or 'dismay' when the word was evoked without the real attitude; much as we say 'I'm sorry' when we have no sorrow, or 'jolly' without real joy. One of our Os (Mo) explained that when she had said "Oh, boy! of all things!" she had experienced no genuine surprise but only "a laryngeal kinaesthesis which stood for the exclamation." Like verbal substitutes are well known to all careful students of pleasantness and unpleasantness.[19] Our method should have distinguished these false attitudes with more care. (3) Our reports show that the observers tended to influence their attitudes by a preceding self-instruction. Here are instances.

> "I must keep calm this time." "I shall work slowly and carefully this time and not get into a muddle." "I told myself in advance of the exposure not to be surprised at anything." (Ht)
>
> "I always work carefully and deliberately." "I tell myself that if I do not get an answer during the exposure I shall not let that fact bother me." (Cu)
>
> "I do not take many of the problems seriously." "What is the use bothering; I don't know the answer and I don't care." (Mo)
>
> "I decided to keep myself calm." (Fx)
>
> "O, dear! I'll probably get one that I cannot solve." "I wish that an easier one would appear this time." (Su)

Just what effect previsory self-instructions of this sort have upon the body at large is doubtful; although they clearly indicate neural trends which direct and modify subsequent operations. They should be taken more carefully into account wherever reports are made by the observer. The disciple of stimulus-and-response also would do well to take his eye off his omnipotent 'environment' long enough to note their great influence upon behavior. The fact that the set or trend may not be subsequently reportable does not imply that it is not effective either in the solution of problems or in the autonomic functions. (4) It appears that some of the attitudes of the checking sort contain no reportable reference to the body at large. They suggest the 'intellective' *Bewusstseinslagen* of the Würzburg school. In a special series of observations (four Os) we made note of this difference in our instructions. Where the body-at-large was reportably involved, the ratio of deflections to no-deflections stood approx.

[19] Cf. P. T. Young, Studies in affective psychology, *Amer. J. Psychol.*, 1927, 38, 169.

2:1; but where no such involvement was noted by the observer the ratio stood approx. 1:2. All of these comments tend to show why we could not reasonably expect a complete correlation between our reported attitudes and galvanometric deflections.

Summary. Our experiments have not substantiated the widespread belief that the galvanic skin reflex is an indicator of some general psychological class, as of emotions, conations or volitions. Most of the experimental work designed to support such a belief has been too gross and too conceptual to merit credence. On the positive side, our study seems to point to a fairly definite functional connection between electrical changes in the skin and certain reportable attitudes (*Bewusstseinslagen*) which mark sudden, decided and momentary checks in the course of the comprehension and the solution of simple problems of an elaborative sort.

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ELEMENTAL VERSUS CONFIGURAL RESPONSE IN THE CHICK

BY M. HOWELL LEWIS

Psychological Laboratory, University of Kansas

By some educators Thorndike's alleged 'laws' of learning have for many years been considered an adequate account of the 'learning process.' Recently Thorndike's assumptions have been seriously criticised both on logical and on experimental grounds. But either because of the inconclusive character of this criticism or because of its limited dissemination, the psychology of learning has remained unaffected in many educational, and in some psychological, quarters. It was the aim of the present study, therefore, to present further experimental evidence upon Thorndike's hypotheses, with special reference to the nature of the perceptual field in learning. In our problem the control of learning was dictated by the principles of Thorndike, on the one hand, and by those of the configurationists, on the other.

The issue of elemental or disparate as opposed to configurational responses is not peculiar to the problems of learning; it is an issue pertaining to the fundamental nature of perceptual stimulation and response. The 'laws' of Thorndike under consideration in this study grow out of a number of assumptions, among them, first, the assumption that a given isolated stimulus will always and inevitably produce the same response and, secondly, the assumption that the 'laws' of learning deal with hypothetical units of behavior designated by the formula $S \rightarrow R$. Behavior thus atomized logically leads us to look for elements in a given situation, and to regard these elements, if found or assumed, as the primary components which we must add together in order to produce and to explain learning. The theory of identical or similar elements is a formulation of the position that response to parts is genetically primary and that, since the organism is able first to select only discrete

elements, learning should proceed from the perceptually simple to the perceptually complex. But if it were established that the immature organism responds primarily to the total situation and secondarily to parts, we might then well inquire as to what, after all, is perceptually simple and what complex. If configural properties are the more primitive, then the alleged atomistic process is working exactly backwards.

An excellent method of comparing the virtues of the two views is the problem of absolute and relative choices when employed either with the human infant or farther down the scale of animal forms.

It was Köhler[1] who first realized the possibilities of this experimental approach. His procedure may be briefly stated as follows. He trained a chick to respond to the brighter of two gray cards, changed after each trial as to position but not in intensity. When the problem was perfectly learned, he substituted for the darker gray a gray brighter than either of the originals. Were it a matter of identical elements, the chick should have still selected the gray previously chosen regardless of the other elements in the complex, even though it had now assumed the relationship of 'darker than.' But as a matter of record, in 86 trials the chick selected the new gray 59 times and the old familiar gray but 29 times. Helson[2] obtained essentially the same but more convincing results with the white rat. His subjects responded correctly to a two-light situation in which, though the relationships were maintained, the absolute intensities as well as the position of the lights were changed after each trial. The immediate antecedent of the present study was an unpublished study of goldfish by R. H. Wheeler. Using a three-light stimulus, Wheeler trained his subjects to choose one absolute feature of a situation and after mastery he shifted all of the lights upward in brightness. This shift he followed by situations in which the lights were made both brighter and dimmer. The evidence indicates that also in goldfish there occurs a response to a total pattern and an almost perfect transfer from situation to situation in which location and intensity were changed but a given detail in relation to other details was preserved.

The present writer has sought to extend and elaborate Wheeler's technique in a like problem with twenty-five chicks. An apparatus consisting of seven identical compartments each illuminated with a 100-w lamp and controlled by a rheostat was built in semi-circular fashion. Food was used as an incentive. The only training consisted in placing the chick at the open end of the apparatus (Fig. 3) and gently directing it for the first day or two toward the compartment lighted at a given intensity. The chicks were 10 days old when the experimenting began.

[1] W. Kohler, *The mentality of apes*, N. Y., 1925.

[2] H. Helson, Insight in the white rat, this JOURNAL, 1927, 10, 378–396.

The investigation consisted of two parts, the first of which was prescribed by Thorndike's assumptions but shifted later to a configurational setting; the second was wholly prescribed by the latter concepts. Two groups of chicks entered the first experiment. For the one group the following program was carried out: (*a*) original training on three absolute intensities, (*b*) three lights shifted upward in intensity, (*c*) five absolute lights, (*d*) five lights shifted both upward and downward, (*e*) five lights and a box (to be described), (*f*) seven lights. The other group was started also on three absolute lights shifted directly to five relative lights and thence to the five-lights-and-box situation. The chicks were kept at these various tasks until they either mastered the task or showed no probability of improvement. In the second part of the investigation, a new group of chicks was trained entirely on five relative lights. The reasons for this will appear as we relate the results.

Results

There were three types of task for the various chicks; choice of the brightest, the dimmest, or the light of middlemost intensity.

The mastery of the three absolute lights, in the first experiment, required about 10 days. Certain of the 'brights' which showed evident photopositive reactions seemed to master the problem immediately; although it soon appeared that this was not the case. Before altering the lights, the 'brights' and the 'dims' were choosing correctly at every trial, and the 'mediums' in at least four-fifths of the trials. The first test of the configurational hypothesis came when the lights were all increased in intensity. Whereas the stereotyped situation had consisted of a B (bright), M (medium) and D (dim), the new situation was composed of a BB (very bright), B, and M. Taking a 'medium' chick, for example, we should expect by the theory of identical elements that, having been trained to M, he should again select M, even though it had now assumed a D-position; or that a 'bright' should select the B though it was now an M with respect to the

total pattern. But as a matter of fact, though the chicks had run to their respective absolute lights as many as a hundred times, the curves suffered but slight depression at the introduction of the new situation. The losses in accuracy were almost immediately retrieved, while in certain cases there was no drop in the curve and in one case it actually rose. Similar depressions occurred four days later when five absolute intensities were introduced; but again there were cases of unchanged accuracy. Almost identical with the previous drops in the curves was the effect seven days later of shifting the intensities of the five lights upward and downward after each trial. While the 'medium' chicks could not manage this situation, the 'brights' and 'dims' evidently could see no difference between these and the stereotyped situations; for, even when every member of the pattern was increased (or decreased) in intensity so that their respective correct lights were alternately very bright or very dim, the responses of these chicks were almost perfect. In the case of the chicks trained on the three absolute lights and shifted directly to the five relative brightnesses, there was a 30-percent fall on the part of the 'brights' and 'mediums,' recovered gradually over a period of 15 days. The 'dims' lost the problem completely but their recovery was twice as rapid.

An extension of Köhler's 'critical test' procedure, which we roughly designated as 'positive' and 'negative progressions,' provided us with a further means of testing the genuine character of the responses. In a negative progression, a situation made up of a number of bright lights which differed only slightly from each other in intensity was presented. The brightest of them was then successively reduced to an intensity of less than that of the dimmest, until all of the lights were very dim. A positive progression proceeded in the opposite direction. Thus the chick would be obliged successively to desert a familiar absolute light, recent both as to location and intensity, for a new light which bore a characteristic relation as a member in the new configuration. The most beautiful examples of behavior of this sort occurred in the six-light situation, composed of five compartments in the semi-circle

and an illuminated box. The latter had an opening, food, and light similar in all respects to the compartments but it could be placed anywhere in the apparatus. Regardless of whether the correct light was bright or feeble, in the box or in the semi-circular compartments, half of the 'brights' selected it perfectly, the other half 60 percent to 75 percent of the time (which is about 50 percent above chance occurrence for six lights). The 'dims' ranged in accuracy between 75 percent and 100 percent. The 'mediums,' however, did so poorly that they were given two lights-plus-the-box; but, though they started out with an 80 percent-performance, they grew steadily worse.

On seven relative lights the 'brights' and 'dims' were able to select the correct compartment in 75 percent to 90 percent of the trials, which is so far above chance as to preclude any doubt as to the genuineness of the choice.

Typical curves of subjects thus far discussed appear in Fig. 1. (Trials were made on five relative lights with the 'brights' and 'dims' 4–7 weeks after the period of experimentation and with no practice in the interval; the chicks then made 100 percent performances.)

With the data thus far secured it seemed safe to assume that reaction-to-a-total-situation was at least as reasonable an hypothesis of primitive response as the notion of discrete elements. Presumably the learning of the stereotyped situations occurred by virtue of the operation of frequency, recency (in intensity, but not in location), partial response, identical elements and other 'laws' of Thorndike. But the fact that the curves did not fall at the destruction of the identical elements sufficiently to indicate that the relative intensities thus introduced constituted a new problem points to the possibility that the absolute intensities may have been of a configurational rather than of a disparate nature. Certainly the reverse is not true. In spite of the fact that the comparison seemed to have been fairly made, with the outcome just observed, a number of questions arise. Why did the curves drop at all? If we attempt to explain the drop by the loss of identical elements, how shall we account for the rapid and

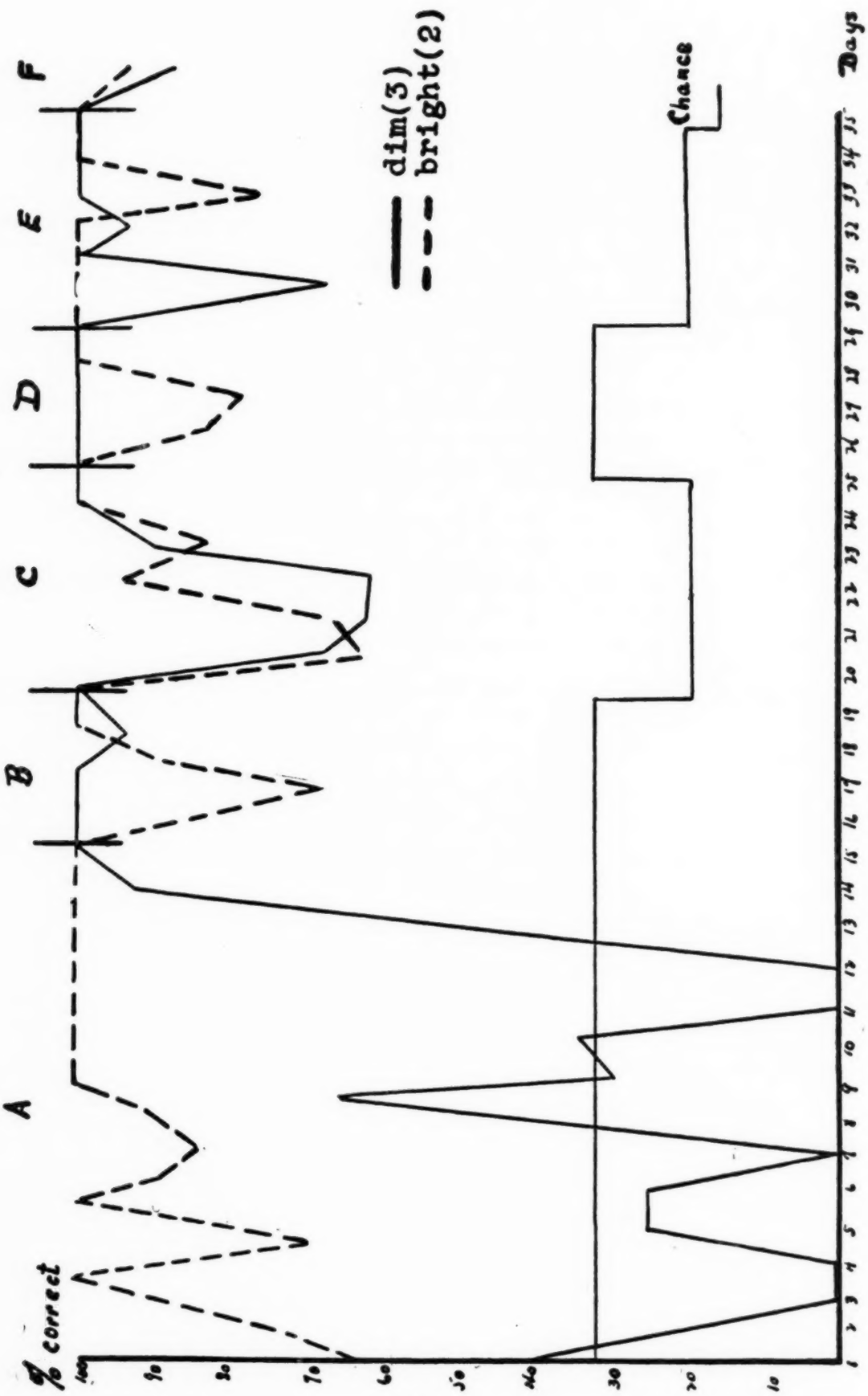

FIG. 1

A = three absolute lights
B = same, shifted upwards
C = five absolute lights
D = five relative lights
E = same, with box
F = seven relative lights

almost immediate recovery of facility in the problem? Can we assume a transfer of criteria? If we say that the correct light regarded as a discrete element was *similar* to the old intensity, do we mean that its intensity was more nearly that of the original light than others of the new situation? Or do we intend that its relation as a detail to other details in the new situation corresponded with its relation to the old situation? Admitting the latter, we have digressed considerably from the atomistic and mosaic conception of the fundamental characteristics of a perceptual complex. Most important of all, however, we are still in doubt as to the nature of the original learning. If a chick actually responds to a total situation in which the relationship which the parts bear toward the whole transcends the individual importance of the parts themselves, then should it be more difficult for the chick to select (say) the brightest light from any situation than it would be to select it from the same situation a number of times? The logic of the hypothesis demands that we maintain it to just this extent. Accordingly, a new group of chicks was secured and the problems were repeated, but now the problems were governed by the configurational theory.

From the very first day, the new chicks were trained on lights increased and decreased in brightness after each choice. In addition, these Ss were started on five lights instead of three. With every conceivable combination of intensities, so that the possibility of acquaintance with identical elements was eliminated, the 'brights' and 'dims' mastered the problem in 8–10 days, or in approximately the same time that it required the first group to learn the three absolute lights and in one-fourth the time for the same tasks by the 'dims' of the first group. The curves of the configurational group shown in Fig. 2 are instructive.

It will readily be seen that the extremes (brightest and dimmest) of the series of light-combinations, involve relationships in but one direction; whereas the relationships of the middle light extend in two directions and hence are double in number. At first sight it would seem that the task of abstracting the common characteristic of middleness from an

apparently heterogeneous succession of lights would transcend the ability of the chick. (I use the word 'heterogeneous' in the atomistic sense, since lights having constancy in neither location nor intensity would be thus considered.) Only by analysis, presumably, could the response be perfected—a higher achievement than is usually accorded the chick. The

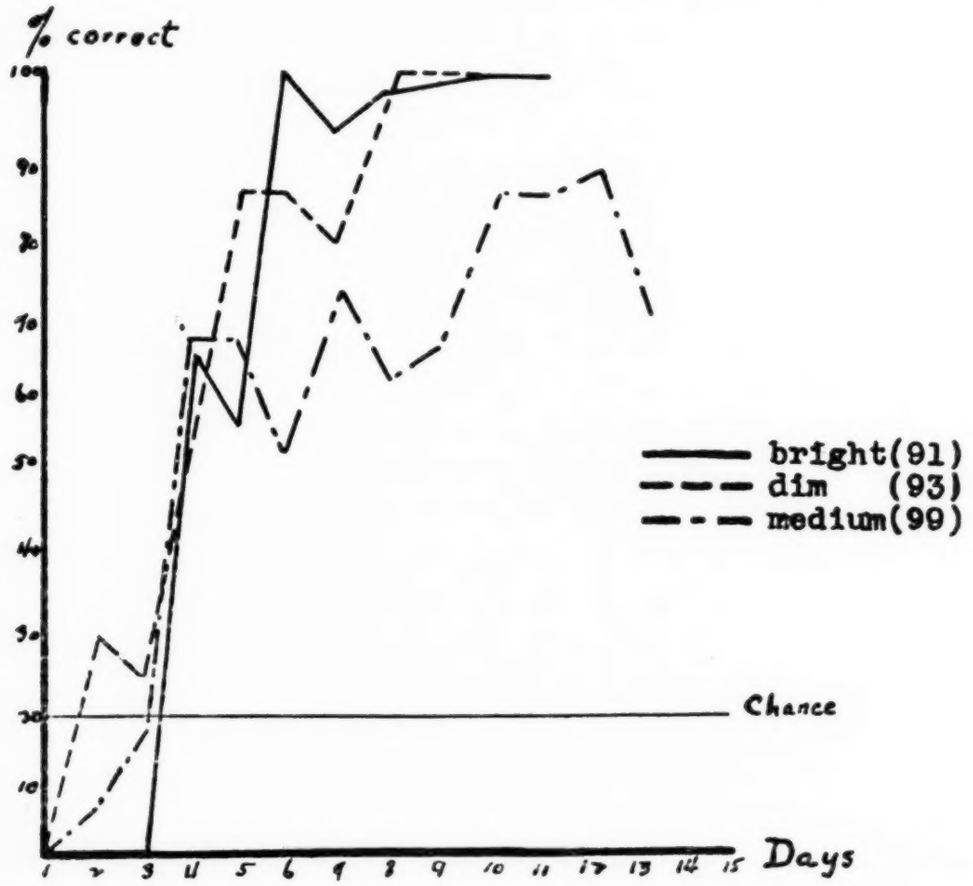

FIG. 2

five-light situation was found to be too difficult for the 'mediums,' so the problem was executed with three. By the eighth day, with the lights altered after each trial, the chicks were choosing correctly 45 percent to 75 percent of the trials. At this point, a significant factor was discovered. Ease or difficulty of discrimination was thought to be determined by the difference in intensity between the various lights. Indeed, for the 'brights' and 'dims' this was wholly true, as the arrangement or geography of the lights had no effect on the per-

formances. With the 'mediums,' however, the geographical factor was found to be as important as differences in intensity. After the eighth day with these chicks, though we continued to present the combinations in pure chance order, we kept a record of the two factors, arrangement and intensity. The choice was greatly facilitated when the correct light was in the center (but not always in the same compartment, since the pattern of lights could be moved from one part of the apparatus to another). When the correct light was at one end of the pattern, it was selected only when the differences in intensity were extreme. One who maintains that the behavior represented no choice at all, merely an 'avoiding' reaction, would find small comfort in the fact that, when the correct light was in the middle with respect to the other two, the chicks made faultless discriminations between differences of light scarcely discerned by the human eye.

Inferences and Conclusions

When we survey the results from all of the subjects, a number of significant observations merit our consideration. *In a majority of the cases there were instances of sudden learning.* Fig. 1 shows but a few of the examples—more striking in the actual observance—of a sudden 'grasp' of the problem. Certain Ss jumped directly from 0 to 100 percent in performance. A second fact, held as equally adverse to a mechanical view of learning, was the abundant evidence of initial delay. In a large number of trials the chick, though hungry, would remain motionless upon being placed in the apparatus and would run to a compartment only after 'surveying' the situation. Indicating that these periods were not merely due to indisposition or to low metabolism, but were states of 'indecision' (using these anthropomorphic terms quite descriptively), we may cite the cases in which the chick would start toward a certain compartment, stop dead still, and then continue either in the same direction *or in another.* The behavior in which the path was altered after apparent original choice strongly implied that the animal was making a definite contribution. The most striking cases of altered choice, when

the configuration was enlarged, occurred in the box-situation when the opening of the box was placed away from the starting point. Time after time a chick would start in the direction of the light which, from his position, was correct; but upon advancing into the apparatus far enough to bring the truly correct light into view—if that light were in the box—he would change his course and eat from the box. A few of the numerous examples of this behavior are presented in Fig. 3. The following expedient with the box furnished us with a convincing bit of evidence against the theory of identical elements. A light of moderate intensity was placed in the box and on succeeding trials only the lights in the semicircle were changed. Thus the box-light, remaining constant, would assume all possible relationships to the total pattern, now the brightest, now medium, and now dimmest; but as it was of the same absolute nature both in position and in intensity, it was an identical element of all identical elements. When the light was correct or incorrect the chicks chose or deserted it, as the case might be, without the slightest 'compunction'; the light changed its identity with the changing conditions related to it.

The case against disparate response as an assumption upon which to build principles of learning is conclusive. The objection has been raised that while the shifting of the lights eliminated those identical elements which were physical and objective, there nevertheless remained intact elements of a physiological and psychological nature. Let us consider the case of a chick directed to the medium intensity of three lights. If we advance equally all of the lights in intensity, will it not be true that the middle light is still physiologically identical even though the gross situation has been recast? (To be willing to consider a 'gross situation' is itself a first concession to the configurational view.) Assuming, however, that the preceding question necessitated an answer (*i.e.* that we had not already discountenanced the theory of identical elements), we constructed situations in which the medium light stood in every quantitative relationship to the other two. Thus at times the medium would be near the bright, the others

B
MB
D
MD
M
chick to medium
DD
MD
B
D
MB
M
chick to bright
DM
BB
MB
M
B
chick to dim

FIG. 3

near the dim. *The results were not altered in the slightest.* The upholders of the theory of identical elements may object that, in our arrangement of lights, we should have been unable to produce elements which remained constant when the total illumination was changed and hence did not give the theory a fair trial. Again, they may argue that by diffusion or other retinal process our attempt to stimulate with identical stimuli must necessarily have proved futile. Be this as it may, we can but compare the performances in situations constructed as carefully as possible of 'identical elements' with comparable performances in settings which were frankly and deliberately planned to eliminate them. So complete was this elimination in our second set of experiments that we might well have expected *no learning whatever* on the strict assumptions of the doctrine which we are attacking.

The deterioration of performance in certain chicks was still more evident from their actual behavior than from their graphic records and suggests the recent contention of Dunlap with respect to the destructive influence of repetition in certain phases of learning. We can expect a learning curve to show decline when a subject is confronted with an alteration of stimuli, however slight. The subject must orient himself; he must accommodate his plan of procedure. And again, if the change of conditions is not radical, we may expect the curve to rise again or at least to maintain the level occasioned by the new situation. Thus, other conditions being equal, repetition should effect improvement or at least maintain the initial state of performance. It is significant, then, that the performance in a majority of our cases grew worse over a period of time. This suggests a criticism of Thorndike's 'law of disuse.' According to his hypothesis, the 'bonds' accounting for or describing the stimulus-response situation are weakened. This disintegration is thought to be a function of, or at least proportional to, the time intervening between the execution of a task and its subsequent repetition. Strangely enough, our series of trials given 4–7 weeks after the period of intensive experimentation was over produced a higher percentage of correct responses than characterized the last of the training

series. The concept of frequency, when used as an explanatory principle in learning, suffers similarly (as has already been indicated) from the cases of sudden learning. Based, as it is, on a philosophy of chance, learning should be characterized in every case by a uniform ascent in a curve indicative of the constant influence of continued repetition upon the perfection of the response and the elimination of errors. When, however, a subject jumps suddenly from complete inability for correct response to complete mastery, our 'frequency' hypothesis breaks down.

When sheer frequency fails as an explanation or condition of learning, those who would retain it as an explanatory concept seek to bolster it up with what has been called the 'law of effect.' Since we have already sinned in the direction of anthropomorphism, we may raise the question as to how we might utilize this principle in treating the facts at hand. By what means did the chicks, when they had become familiar with the task, select the peculiar aspect of the situation to which they were directed. Shall we say that the pleasure derived from eating, from the completed response, or whatnot, was transformed into a selective agency and that it subsequently 'stamped in' the connection between the particular light or light-gradation and the characteristic response? And if dissatisfaction 'stamps out,' so that there is no learning in the unpleasant situation, we wonder that the chicks directed to the bright intensity ever were 'willing' to choose in this direction where the light was made almost dazzling and 'uncomfortably' intense. That this degree of illumination was annoying to the chicks was unmistakable; two of them were virtually paralyzed in the compartment. Yet every 'bright' subject chose the extreme intensity when it was introduced. If the law of effect is stated in a way which merely indicates that the processes of learning have, as a general rule, certain algedonic concomitants, then it is certainly innocuous enough; but if the law is designed to explain the manner or to reveal the mechanism or principle by which the animal learns a problem, we can find no justification for its application to the behavior herein presented.

The whole problem of the obscure determinants of learning is still in the speculative stage. Explanations proposed have not advanced far beyond the sphere of tautology and they have been, for the greater part, more logical than psychological or physiological. In this study we have been concerned primarily with perceptual factors as they operated in a total stimulus-response-configuration, and not with the organic or structural modification which must necessarily occur. Certain aspects of the 'inner' behavior were so prominent, however, that we have ventured to present them in order to complete our account. We have already mentioned a reason for the initial delays, so numerous in the performance of our chicks. It would be presumptuous to infer that during this period the chick was 'considering the next thing to do' or that he was 'analyzing the situation at hand.' Very likely Köhler has attributed to chicks a greater degree of insight or ideational behavior than is justified. But the chick's contribution cannot be denied. Our description of the choices is presented as a behavioristic account of the observations. One may place one's own interpretation upon them or apply one's own hypothesis. Whether one uses 'consciousness' or some other questionable term, the part played by the animal is unmistakable. Of the various speculations proposed as to the nature of this organism's contribution, one thing seems clear. It is a fact that this type of behavior cannot be described or explained in mechanical terms. Possibly a machine will some day respond to a configuration; but with the ordinary (and philosophical) meaning of the word mechanical (implying as it would in our problem, a theory of sensitivity to discrete physical elements), we are unable to conceive of any way in which it might fruitfully be used. The many instances of initial delay, sudden learning and altered direction of movement after initial choice, convince us that, while the actual factors involved may as yet be obscure, we are certainly unjustified, and indeed, unable, to envisage the procedure in terms of abstract and disparate 'identical' elements.

Summary

Accepted principles of learning are, at present, going through a transitional period. The theory of behavioral development as a concatenation of discrete units, which gave rise to Thorndike's formulation of the 'laws' of learning, is now undergoing modification in the direction of an organismic and configurational view. A basic issue has been the problem of primitive sensitivity, whether it be to discrete elements as such or to total patterns. In an attempt to present further evidence on this question, we trained a group of chicks to respond to intensive differences by selecting the brightest, middlemost, or dimmest light from a group of 3–7 lights.

The experiment consisted of two parts. In the first, the conditions of the original training were prescribed by Thorndike's 'laws' and, after mastery of the task, were changed to conditions set up by the configurational hypothesis. In the second part, the conditions of entire learning were configurational in nature. Accordingly, chicks were first trained to select the correct light of three absolute intensities altered after each trial in position only, and were then presented with lights of relative character from which they were expected to select a given characteristic independent of the absolute intensity values. Almost perfect transfer was found to prevail when the situations were changed.

A second group of Ss were trained to respond to five lights, each of which was changed from the beginning both in location and in intensity after each trial. This group mastered the five relative lights in the same time necessary for the first group to master three absolute lights and, with certain exceptions, in half the time necessary for mastery by the first group of a similar task.

The evidence indicates that neither original learning nor transfer of training depends of necessity upon the notion of 'identical elements,' as assumed in Thorndike's 'laws.' The numerous observations of sudden learning, initial delay and evident choice-behavior point to the increasing usefulness of the configurational hypothesis in the treatment of perceptual response.

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BACKWARD CONDITIONING OF THE LID REFLEX

BY ST. CLAIR A. SWITZER[1]

Miami University

The phenomenon of backward conditioning has been examined by Pavlov (9, 27) and Cason (4, 191). Since these experiments have served as points of departure for the present study, a survey of their results will throw some light upon the nature of the problem.

Pavlov refers to the failure of Krestovnikov to secure a conditioned salivary reaction in dogs, either defensive or alimentary, when the unconditioned stimulus preceded the conditioned. He says that "the result was always negative when the neutral stimulus was applied, whether 10 seconds, 5 seconds, or only a single second after the beginning of the unconditioned stimulus." On this basis he concludes that

> "a first and most essential requisite for the formation of a new conditioned stimulus lies in a coincidence in time of the action of any previously neutral stimulus with some definite unconditioned stimulus. Further, it is not enough that there should be overlapping between the two stimuli; it is also and equally necessary that the conditioned stimulus should begin to operate before the unconditioned stimulus comes into action. If this order is reversed, the unconditioned stimulus being applied first and the neutral stimulus secondly, the conditioned reflex cannot be established at all" (9, 27 ff).

But in the Croonian Lecture delivered before the Royal Society on May 10, 1928, Pavlov, with his customary fidelity to the results of experiment, reversed his former position in the following statement.[2]

> "A full and stable conditioned reflex develops when the stimulus which is to become conditioned slightly precedes that activity (unconditioned reflex) with which it is linked. The stimulus may, also, without ill-effect, terminate a short time before the activity begins (conditioned trace reflex); *but if the stimulus is introduced after the*

[1] This study was conducted in the Psychological Laboratory of the University of Wisconsin. The writer wishes to express his indebtedness to Professor Clark L. Hull both for valuable aid in designing the apparatus and for helpful suggestions and criticisms during the course of the investigation.

[2] The present investigation had been completed before these more recent experiments of Professor Pavlov were brought to the writer's attention. The writer has added italics in the quotation.

beginning of the activity, then, although, as our present experiments seem to show, a conditioned reflex may also develop, it is insignificant and evanescent; on continuing the procedure the stimulus, which in this connection we term the neutral agent, becomes inhibitory" (10, 381 ff).

Humphrey, in a critical analysis of Percival Symonds' "twenty-three descriptions or laws of learning," (14) based on Pavlov's investigations of the conditioned reflex, remarks that "Professor Symonds' third law, which states that the conditioned stimulus cannot come after the unconditioned one, also deserves comment. Beritoff, another of Pavlov's co-workers and a neurologist of note, claims to have obtained with pigeons results that directly contradict the view of Pavlov's school that such reversed conditioned responses are impossible. Whether or not backward conditioned reflexes are possible in the normal human being, experiment alone must decide. It is certain that they would, at least, be much more difficult to establish, so that Professor Symonds' practical statement of the principle is probably justified" (8, 428).

Cason has published an account of a simple experimental set-up for investigating backward conditioning of the lid reflex with human subjects. As this, together with Cason's article on the pupillary reflex (5), constitutes the only recorded attempt to test the phenomenon of retroactive conditioning in human beings, and as it deals with a simple defense-reaction which offers a favorable experimental approach, the procedure is of especial interest. Only one subject was used. A telegraph sounder was connected with the wink apparatus, so that every time the subject winked naturally the sound was given automatically. The sound-stimulus occurred a short time after each spontaneous wink, or, in Cason's words, 'when the natural wink was practically completed.' The subject sat in the apparatus for five hours and did not respond to applications of the sound-stimulus delivered by the experimenter at the end of the period (4, 291).

In his classical study of memory, Ebbinghaus found that "as a result of the learning of a series, certain connections of the members are therefore actually formed in a reverse as well as in a forward direction. These connections are revealed by the fact that series which are formed out of members thus connected are more easily learned than similar series whose individual members are just as familiar but which have not been previously connected" (7, 112–113).

Cason states, in a discussion of Ebbinghaus' study, that a serious difficulty in regard to the physical mechanism involved in backward association lies in the law of forward direction across the synapse. Although there is evidence of centrifugal sensory paths, he concludes that the existence of these paths should probably not be taken as evidence favoring the possibility of backward association (3, 221).

Yarbrough (16, 12), in reviewing the work of Wohlgemuth (15, 447), Carr (1, 335–353), and Carr and Freeman (2, 465) on backward association in animal and human learning, summarizes the results of these investigators as follows. "All investigators have found a tendency toward backward association—a tendency varying in strength, but seldom equal to and never stronger than the forward tendency." Yarbrough's own experiments with white rats led him to conclude that "association in the backward direction is very little, if any, more difficult than in the forward direction when the continuous mode of presentation is used. If, however, the two terms to be associated are presented with a time interval of one second between them, the association in the reverse direction is perhaps impossible" (16, 51).

Several implications of the studies of Krestovnikov and Cason are to be later examined in detail in this paper.

Various factors in the investigations cited above made it seem advisable to attack the problem with a different technique, incorporating a series of controls. These considerations led the writer to attempt the present study, the primary object of which was to determine the possibility of establishing a backward conditioning of the lid-reaction in human beings. Short supplementary studies were also made of the patellar reflex and galvanic skin reflex.

The Experiments

Apparatus. Closure of the lids may be caused by a very bright light entering the eyes, by the sudden approach of some foreign body, by contact of a foreign body with the lashes, and by irritation of the cornea or conjunctiva (13, 514). The original unconditioned stimulus evoking the wink reflex was probably contact of some foreign body with a part of the eye or the tissue immediately surrounding the eye. It was decided, therefore, to employ a small electrically released hammer (see Fig. 1) as the unconditioned stimulus in this investigation.

This wooden striking hammer was poised in front of the eye, slightly above the pupillary center. The point of stimulation or contact was one-half inch below the left eye of the subject. The hammer (*B*) was padded slightly with cotton covered with a small silken cap at the striking end (*A*), and was released by an electro-magnetic attachment (*G*) connected with four dry cells. The experimenter produced the stimulation by closing an electric switch on the table with his left hand and returned the hammer to its original position by operating a connecting cord (*F*) with his right hand. The apparatus was so arranged and screened that no movement of the experimenter was visible to the subject at any time. The distance of the hammer from the subject's face was kept constant (2 in.) in every case in order to insure uniformity of stimulation.

The winking movements were recorded on the kymograph by means of a specially constructed tracing device (see Fig. 2). The arms of this recorder were made from 5/16-in. square brass tubing.[3] One end of the lower arm (*I*) was soldered securely to the vertical arm (*E*) and the other end was clamped to the ring-stand.

The upper horizontal arm (*C*) and the vertical arm were connected by a soldered X sleeve (*D*) of 6/16-in. brass tubing through which they could be moved with a little effort. This made a standard which allowed both vertical and horizontal adjustment of arm (*C*), providing a method for reducing any slack that might occur in the response-transmission thread (*B*). A very delicate spring (*H*) was attached to the base of the vertical arm of the recording device. A fine silk thread (*B*) was fastened to the top end of this spring and passed from there to a very light glass recording needle (*G*),

[3] The writer is indebted to Mr. W. S. Ray for the design of the adjustable framework of the recording device.

3½-in. in length, which was hinged to a projecting wire (*F*) fixed to the vertical arm 3 in. above the base. The silk thread was secured to the glass recording needle at a point one inch from the hinged end, and then passed upward through an eye in the X sleeve and along the lower side of the upper horizontal arm through an eye at the outer end, and thence to the right eyelid of the subject.

A small piece of aluminum foil (*A*) was attached to the free end of the silk thread. This foil was glued to the lower part of the subject's right eyelid, just above the lash. As the lid fell, the glass needle was pulled upward, its point tracing the movement on the smoked paper, at the same time magnifying it 3½ times. The bronze spring prevented excessive coasting and brought the needle back to its original position following each lid-movement. This apparatus was so light that the subjects no longer felt it when it had been attached a few seconds.

Other accessory apparatus included a Morris chair,

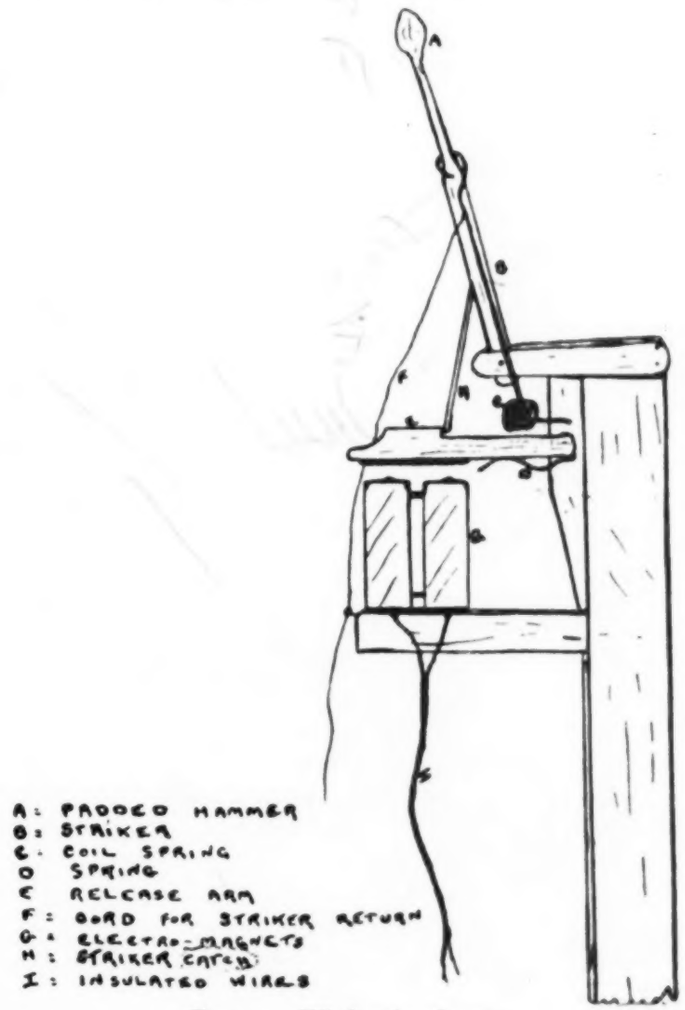

FIG. 1. Wink stimulator.

Stoelting extension-kymograph, buzzer, a two-knife switch, two single-knife switches, two electro-magnetic signal markers, electro-magnetic time marker connected in circuit with the

laboratory time-clock, optical fixation-disk, and Whipple stop-clock.

One signal marker was connected with the switch operating the buzzer, and the other with that operating the wink stimulator. The optical fixation disk was fastened to the wall directly opposite the subject and at a point above his ordinary level of vision as he sat in the chair. Instructions to fixate the eyes upon the disk kept the subject from anticipating the fall of the hammer and maintained the lids in such a position as to insure an objective record of their smallest movements.

Procedure. The subjects were divided into two groups; (1) the Experimental Group (8 men and 12 women), and (2) the Control Group (4 men and 6 women). Two additional

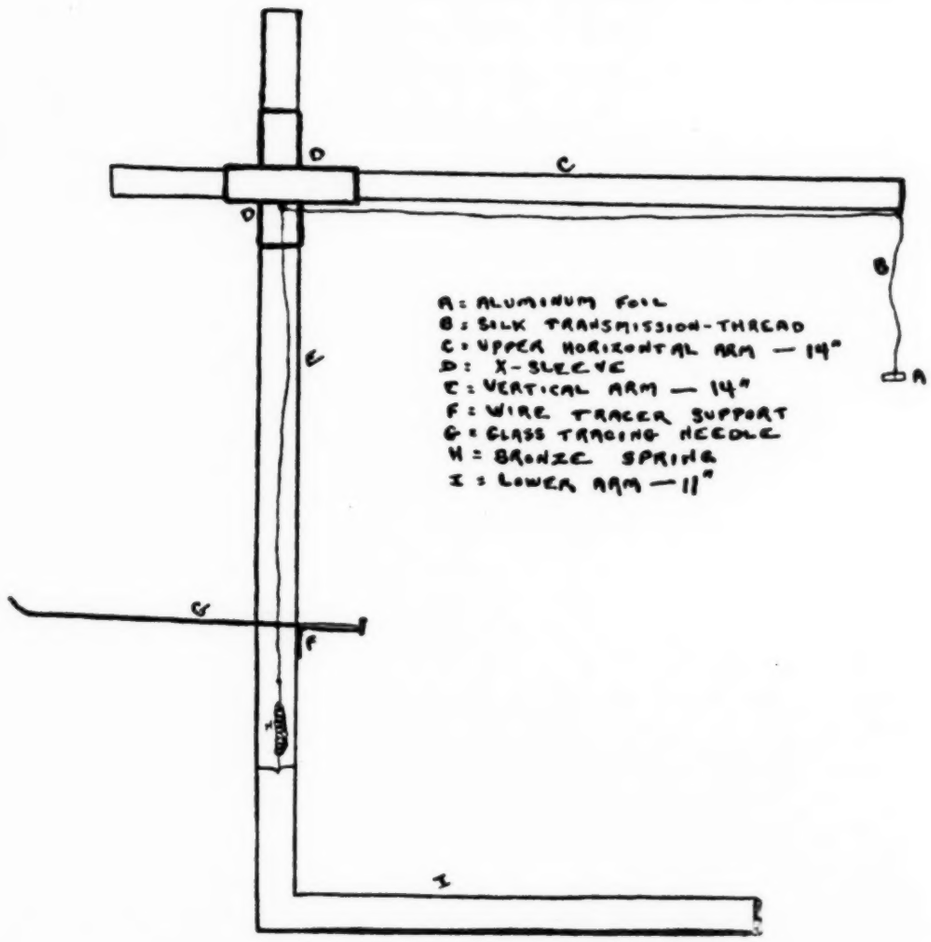

FIG. 2. Wink-recording device.

subjects, one man and one woman, were classified as the Control-Experimental Group.

It was necessary to consider two factors in selecting suitable subjects. (*a*) An individual was considered unsuitable if the buzzer caused him to wink twice in succes-

sion when it was sounded at the beginning of the experiment, and (*b*) an individual with an excessive normal winking rate was also considered unsuitable because of the fact that, under the conditions of the experiment, it would be impossible to distinguish his spontaneous winks from real conditioned responses.

All subjects were university students. Approximately one-half knew nothing about the principle of conditioned reactions.

The room was electrically lighted after the apparatus had been adjusted, the lights were dimmed to secure the minimal amount of eyestrain, thereby materially reducing the number of spontaneous winks. The general plan of procedure was the same at all times, except for the following necessary variations between Experimental and Control Groups.

For the Experimental Group the aluminum foil of the wink-recording device was attached to the subject's right eyelid with a drop of glue. During the minute or two which was allowed for the glue to set, the tracing points were adjusted to the kymograph in a straight vertical line.

The subject was next given the following verbal instructions:

> "Take as relaxed and comfortable a position as possible, with your head resting against the back of your chair. Avoid thinking about the experiment while it is in progress. You may solve a mathematical problem, count backwards, recite a poem, or perform any other silent process which will keep your thoughts occupied. Try not to move your head at any time. Fixate your gaze upon the round disk on the wall; but do not allow your eyes to become strained by looking too long at this object."

The slack in the silk transmission-thread was then taken up by adjusting the arms of the recorder. The stimulator was adjusted so that the padded end of the hammer was just two inches away from the face of the subject. The blow of the striker was delivered at the lower edge of the eye-socket, about one-half inch from the naked cornea. The subject was always aware that a slight downward movement of his head would cause him to receive a painful blow on the eyeball.

The kymograph was started and allowed to run for 3–4 minutes in order to secure the normal winking rate of the subject. The buzzer was then sounded alone several times. Sufficient trials were made with the sound in each case to establish the fact that the sound was a neutral stimulus, *i.e.* did not cause a wink at this stage of the experiment. (Thus line *C* of Fig. 3 shows five buzzer stimulations at the outset without any accompanying wink on line *A*.)

The training series consisted of paired stimulations with striker and buzzer. The conditioned stimulus (buzzer) always *followed* the unconditioned stimulus (striker) after an interval of ½–2 sec, the time for any given subject always being constant. That the conditioned stimulus follows the unconditioned stimulus is shown in the record (Fig. 3) by the fact that the buzzer signal is always slightly to the right of (later than) the striker signal (line *B*). The backward conditioning time

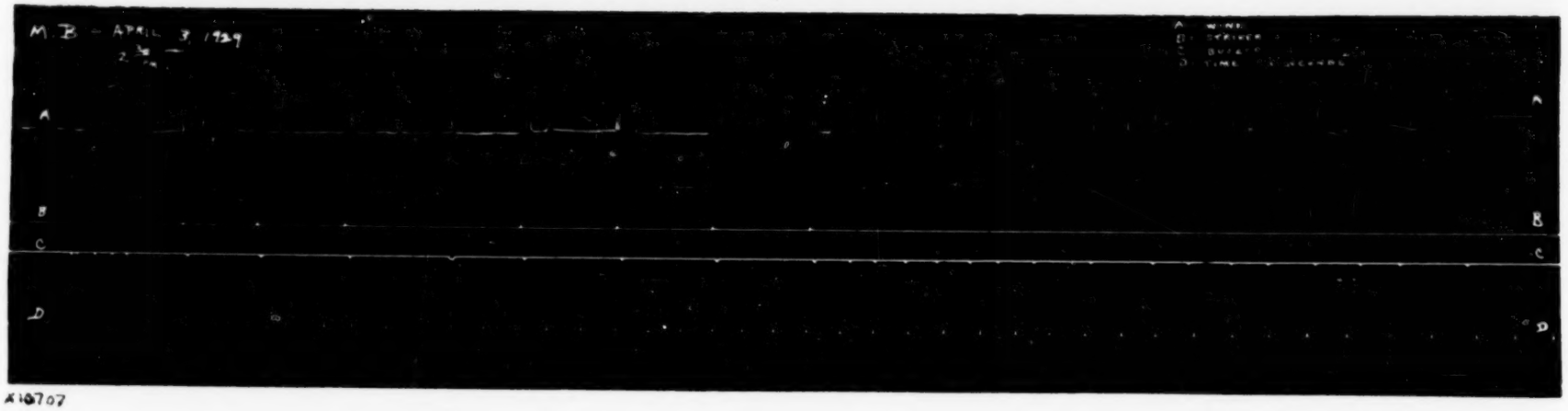

FIG. 3. Backward conditioning of the eyelid reaction. Line *C* shows five buzzer stimulations at the outset without an accompanying wink on line *A*. The buzzer signal (line *C*) is always slightly to the right of (later than) the striker signal (line *B*).

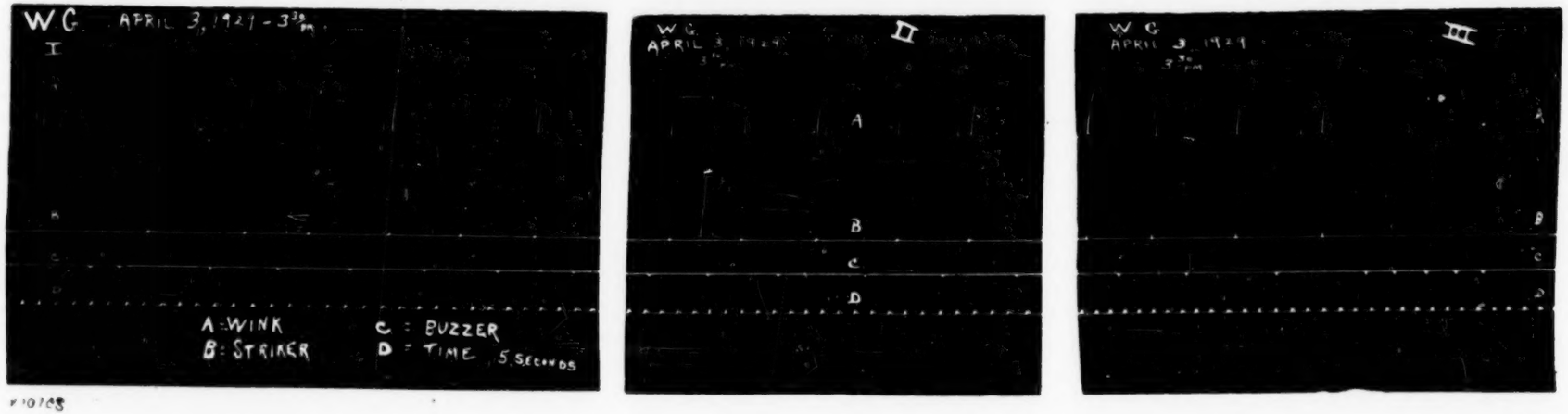

FIG. 4. The beginning, center and final sections of a Control record. Time between Section I and Section II = 7½ minutes. Re-enforcements between Section I and Section II = 22. Time between Section II and Section III = 4 minutes. Re-enforcements between Section II and Section III = 11. Interval between Striker (line *B*) and Buzzer (line *C*) average = 10 seconds.

interval used with each individual subject is given in Column 3 of Table 1. The buzzer was never sounded until the wink

TABLE I

TIME-INTERVALS BETWEEN THE CONDITIONED AND THE UNCONDITIONED STIMULI, AND VARIATIONS IN RESPONSE, FOR SUBJECTS OF THE EXPERIMENTAL AND CONTROL-EXPERIMENTAL GROUPS

	Subjects*	Normal winking rate per minute	Interval between uncond'ed and cond'ed stimuli (sec)	No. reenforcements to produce 3 or more successive cond'ed responses to buzzer	Ave. ampl. (mm) of first 10 re-enforcements (striker followed by buzzer)	Ave. ampl. (mm) of first 10 cond'ed responses
	1	2	3	4	5	6
Experimental Group	I	6	1	20	20.6	3.4
	II	2–3	2	11	20.7	5.3
	III	1–2	2	14	23.8	6.8
	IV	1–2	2	—	24.6	—
	V	4	2	—	29.6	2.0 (2 C.R. only)
	VI	21	½	20	26.4	3.4
	VII	8	1	17	28.8	2.4
	VIII	8	1	17	31.2	7.3
	IX	6	1	24	31.7	3.2
	X	11	1	20	41.4	8.6
	XI	4	½	46	41.8	3.3
	XII	12	½	25	25.9	1.9
	XIII	2–3	1	47	20.4	7.6
	XIV	6	½	23	23.9	3.7
	XV	9	1	62	21.4	2.2
	XVI	8	¾	20	20.6	7.9
	XVII	8	1	12	24.2	18.0
	XVIII	9	1	30	24.8	11.9
	XIX	8	1	58	20.1	3.8
	XX	3–4	½	7	16.0	10.8
Control-Experimental Group	I	8	½	62	17.4	2.4
	II	10	1	48	25.0	1.0

* Detailed individual tables for all subjects are on file in the Library of the University of Wisconsin at Madison.

caused by the striker had registered on the smoked paper, and the hammer had been returned to its original position. The interval between the pairs of stimuli (striker followed by buzzer) was purposely varied between 10–20 sec at different points on each record in order to guard against a temporal conditioning (9, 41).

First evidences of the onset of conditioning were usually

manifested by the appearance of double winks for the paired stimuli—a complete wink or closure for the striker followed by a partial closure for the buzzer. When this phenomenon appeared, the experimenter sounded the buzzer alone to determine whether the conditioned reflex had been established. Three or more consecutive responses to applications of the buzzer alone were accepted as evidence that a conditioned eyelid-reaction to the sound had been established.

The forward interval between the buzzer of one pair of stimuli (striker followed by buzzer) to the striker of the succeeding pair was always 20 times as long as the backward interval in any given pair. It was regarded as possible, however, that the conditioning was forward over this longer interval rather than backward over the short interval. Accordingly, a control experiment was set up to determine whether under the conditions of the main experiment forward conditioning would take place over such a long interval. The subjects of this experiment were called the Control Group. With them the procedure was exactly as given above except that *instead of pairing the stimuli (striker-buzzer), each was sounded alone, alternately, at intervals varying from 8 to 15 sec* (see Fig. 4). This separated the unconditioned and neutral stimuli in such a way as to test whether or not the buzzer (neutral) might be substituted for the striker (unconditioned), thus establishing a conditioned lid-reflex, over an interval *somewhat shorter* in every case than the interval between combinations with the Experimental Group. If conditioning failed to take place under this arrangement, when the interval between buzzer and striker was *shorter* than in the original experiment, it seemed improbable that it might have operated over the longer interval which was used between combinations in the Experimental Group. At the end of training series equal in length to those required to establish a conditioned reaction in subjects of the Experimental Group, the buzzer was sounded alone a number of times. If no reaction occurred, the fact was accepted as evidence that forward conditioning had not been established.

That the conditioned reactions secured in members of

the Experimental Group were not attributable to the phenomenon of irradiation (9, 152 ff.) was made quite clear by the fact that an irrelevant bell stimulus in the adjoining room failed to take on the properties of a conditioning agent on three separate occasions. (See note on Subjs. iii and ix below.)

TABLE II

TIME-INTERVALS BETWEEN CONDITIONED AND UNCONDITIONED STIMULI, AND VARIATIONS IN RESPONSE, FOR SUBJECTS OF THE CONTROL AND CONTROL-EXPERIMENTAL GROUPS

	Subjects	Normal winking rate per minute	Interval between uncond'ed and cond'ed stimuli (sec)	Ave. ampl. (mm) of first ten winks to the striker (uncond'ed stimulus)
	1	2	3	4
Control Group	1	7	15	14.7
	2	16	12	28.1
	3	8	15	22.7
	4	5	8	29.5
	5	2–3	12	12.0
	6	7	10	21.3
	7	4	10	25.0
	8	9	12	17.2
	9	2–3	15	24.1
	10	5	12	17.8
Control-Experimental Group	I	8	12	24.7
	II	10	15	23.6

The Control-Experimental Group consisted of two subjects who were first subjected to Control-Group procedure and then subjected immediately afterward to the Experimental-Group technique. It was thought that if the same individuals failed to acquire the conditioned reflex under the Control-Group procedure and then immediately after acquired it successfully by the Experimental-Group method, the possibility of a forward conditioning in the Experimental Group, as an explanation of the conditioning found, would be doubly precluded.

RESULTS AND DISCUSSION

A. In the Experimental Group, three or more consecutive winks to the buzzer alone (after the training period) were

taken as evidence that a conditioned eyelid-reaction had been established to the sound-stimulus which had previously been neutral.

Backward conditioning was established in eighteen (90%) of the subjects of the Experimental Group. Subj. v (RS) gave two responses to the buzzer alone after a short training period but ceased to respond in the later stages of the experiment. He afterwards informed the experimenter that he had

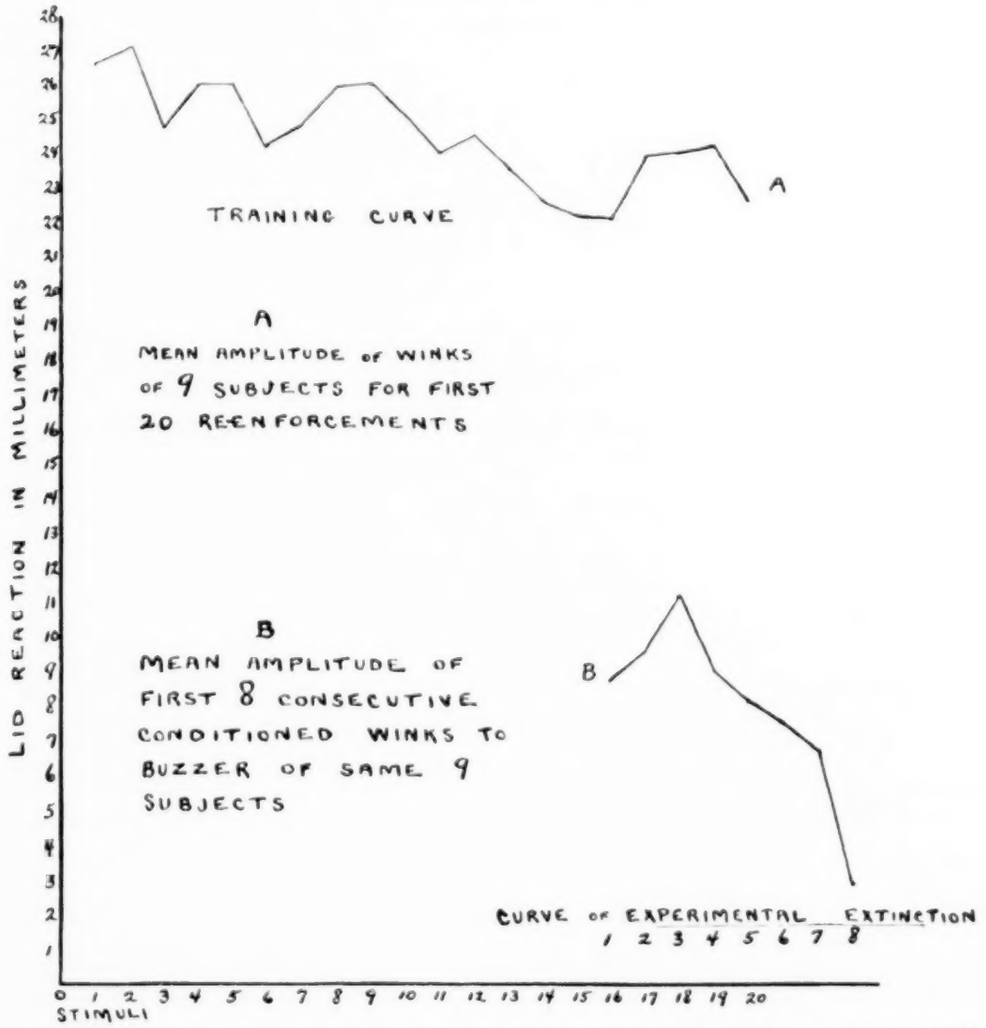
LID REACTION IN MILLIMETERS
STIMULI
TRAINING CURVE
A
A
MEAN AMPLITUDE OF WINKS OF 9 SUBJECTS FOR FIRST 20 REENFORCEMENTS
B
MEAN AMPLITUDE OF FIRST 8 CONSECUTIVE CONDITIONED WINKS TO BUZZER OF SAME 9 SUBJECTS
B
CURVE OF EXPERIMENTAL EXTINCTION
1 2 3 4 5 6 7 8

FIG. 5. Amplitude of winks during training period and during period before experimental extinction reached zero. (Curve of experimental extinction for balance of Exp. Group shown in Fig. 6.)

worked for two years in a telephone office and had become negatively adapted to a buzzer of similar intensity to the one used in this study.

Table III

Amplitude of winks of 9 subjects during training period and during the period before onset of first experimental extinction

Subj.	First 20 re-enforcements																				1st 8 consec. cond'ed responses							
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	1	2	3	4	5	6	7	8
I. . .	17	22	25	22	19	18	15	17	30	21	22	19	20	24	7	11	24	16	23	11	4	1	6	8	0	4	2	2
III. . .	23	26	23	24	21	23	27	27	24	20	16	18	9	10	20	13	18	19	21	15	4	5	3	4	4	6	8	2
IX. . .	42	31	28	32	37	23	32	29	30	33	33	31	31	30	29	28	28	28	28	30	3	5	11	1	2	2	4	2
X. . .	42	48	39	48	44	40	39	39	40	35	31	38	35	35	36	37	39	37	37	36	9	13	15	7	11	3	2	14
XII. . .	26	26	25	27	26	26	25	25	25	28	20	26	24	22	27	25	25	25	21	26	14	7	8	13	0	7	5	0
XIII. . .	27	24	17	19	20	20	20	20	20	17	21	19	28	19	19	21	18	19	21	23	7	14	15	10	17	14	6	1
XIV. . .	31	25	27	22	25	25	26	22	15	21	23	20	22	19	18	23	18	25	19	18	9	12	10	11	12	0	5	0
XVII. . .	23	24	21	26	24	25	21	28	24	26	26	25	20	22	23	20	22	—	—	—	20	20	22	20	20	23	17	0
XX. . .	9	18	17	14	18	18	18	—	—	—	—	—	—	—	—	—	—	—	—	—	11	11	13	10	10	11	14	7
Total. . .	240	244	222	234	234	218	223	207	208	201	192	196	189	181	179	178	192	169	170	159	81	88	103	84	76	70	63	28
Mean. . .	26.6	27.1	24.7	26.0	26.0	24.2	24.8	25.9	26.0	25.1	24.0	24.5	23.6	22.6	22.4	22.3	24.0	24.1	24.3	22.7	9.0	9.8	11.4	9.3	8.4	7.8	7.0	3.1

Variations in the number of combined stimuli necessary to establish three or more successive lid-reactions to the buzzer alone are shown in Table I. All reactions were measured in mm. These conditioned winks varied in amplitude from the smallest observable movement of the lid to a wink of approximately the same amplitude as the subject's unconditioned lid-reaction.

TABLE IV

COMPOSITE OF AMPLITUDES OF WINKS RECORDED FOR FIRST 5 CONSECUTIVE STIMULATIONS WITH BUZZER AFTER CONDITIONING HAD BEEN ESTABLISHED*

		Winks				
Subj.	Stim'n	1	2	3	4	5
II		8	13	13	0	0
VI		6	8	5	5	9
VII		1	1	6	0	6
VIII		19	14	13	0	0
XI		0	6	3	3	3
XV		6	0	12	7	0
XVI		10	11	9	3	0
XVIII		22	21	21	0	0
XIX		3	7	8	4	1
Total		75	81	90	22	19
Mean		8.3	9.0	10.0	2.4	2.1

* (The conditioned response was less stable in these subjects than in those shown in Table III.)

Typical verbal reports:

Subj. vii (DW): "I didn't notice the buzzer at all at the beginning of the experiment, but after I had heard it a few times I noticed that I was winking whenever it sounded, so I tried to keep from winking when I heard it for a while. At first I thought the buzzer was an instrument marking a time interval, and then I noticed that it didn't sound at regular intervals. After a while I thought it might be buzzing whenever I winked, so I tried winking a few times voluntarily; but it didn't buzz then. I never thought about the buzzer's being connected with the hammer striking my face."

Subject x (LR): "I was scared when the buzzer first sounded alone after the other thing had been hitting me, but after the buzzer sounded a few times it didn't bother me any more."

Two incidents furnished some evidence that conditioned reactions to the buzzer were not due to the phenomenon of irradiation.

(1) During the training period of Subj. iii (FG), the steam radiator, which was situated directly behind the subject's chair, began pounding loudly. The experimenter observed the subject closely through the vent provided in the screen. This radiator noise did not cause S to wink, so the experimenter introduced two separate applications

of the buzzer alone. S winked to this conditioned stimulus in each case, and this in spite of the fact that the stimulus was barely audible on account of the noise made by the hammering of the radiator.

(2) Following the training period of Subj. ix (IS) a loud electric bell was sounded several times in an adjoining room. S failed to respond. A buzzer sounded in the next room, however, evoked the wink-reaction as readily as the buzzer used in this experiment—the two buzzers being of equal intensity according to the report given afterwards by S.

B. In the Control Group all ten Ss failed to show any response at the end of the training period, furnishing evidence that the results obtained for the Experimental Group were not due to forward conditioning.

Subj. 2 (OE) gave one response to five applications of the buzzer alone at the end of the training period. As this S had a high normal winking-rate (averaging 16 spontaneous winks per minute) this single coincidence of buzzer and lid-movement is not significant.

C. In the Control-Experimental Group both Ss were successfully conditioned to the buzzer under Experimental-Group procedure after failing to respond to this stimulus at the end of a Control-Group training period, thus corroborating the results obtained for the main experimental groups. As the control period was relatively short in each case, however, no very great significance can be attached to the results obtained by this method.

It will be recalled that Dr. Krestovnikov failed to secure backward conditioning of the salivary reflex in numerous experiments with five dogs. There are several factors involved in Dr. Krestovnikov's investigation which may account for his failure to secure the phenomenon. In the first place, certain reflex actions, such as the alimentary reflex, the galvanic skin reflex, and others, involve a fairly long response. During the phase of secretion of saliva in the alimentary reflex, other stimuli, particularly those characterized as external, are usually rendered neutral so far as that particular response is concerned. When the response has been completed, the time interval between the unconditioned and the neutral stimuli may be too great to allow any connection to be formed. Secondly, the fact that the alimentary reaction is not a defense-reaction probably is the greatest argument against its use in the study of backward or retroactive conditioning. The evidence seems to indicate that this form of conditioning occurs most readily in cases where defense-reactions are involved.

In the above connection we must inquire into the failure of Dr. Krestovnikov to secure retroactive conditioning of the *defense* salivary reaction. In this case he introduced acid (the unconditioned stimulus) into the dog's mouth some 5–10 sec before he presented the odor of vanillin (the conditioned stimulus). Here we have an example of a strong defense-reaction which has a relatively long period of response. It is quite

safe to say that 5 to 10 seconds is scarcely sufficient time for recovery from the effects of even a mild solution of acid placed in the mouth. The powerful effort to eject the acid may have rendered all extraneous stimuli neutral, or practically so, for the time being (12, 226–234). The possibility of establishing a conditioned reaction may have been eliminated by the conflicting factors of a long refractory phase and a short interval between the unconditioned and neutral stimuli.

It must be remembered, also, that Dr. Krestovnikov employed only dogs in his investigation. It is possible that human subjects may react differently.

The recent, and as yet unpublished, experiments of Professor Pavlov and his staff furnish evidence of the possibility of establishing backward conditioning of the salivary reflex in dogs; although he believes conditioned reflexes thus formed to be insignificant and evanescent (10, 381 ff). In the present investigation, however, Subject iii (FG) was tested after 48 hours for retention of the conditioned lid-reaction formed in this manner, and it was found that he responded to the buzzer alone 16 out of 24 times (66.7 percent) without re-enforcement with the striker after the 48-hour interval. After 3 re-enforcements with striker followed by buzzer, he gave the lid-reaction to 19 out of 22 (86.4 percent) applications of the buzzer alone. (See also Table III for additional cases of resistance to experimental extinction.)

It will be noted from an examination of Fig. 6 and curve *B* of Fig. 5 that the conditioned reaction does not reach its maximal amplitude until the third or fourth application of the conditioned stimulus. The two curves (Fig. 6 and curve *B* of Fig. 5) represent the conditioned reactions of *two independent sets of subjects*, each set comprising one-half of the main Experimental Group. In the curve of experimental extinction of backward or retroactive conditioned reflexes, there is a clearly marked tendency towards a sharp rise during the first few applications of the conditioned stimulus, followed by a more or less rapid falling off toward the point of extinction, or zero. This tendency was observed to be comparatively constant for all subjects tested.[4]

Curves *A* and *B* of Fig. 5 show the amplitudes of the unconditioned and conditioned responses, respectively. It will be noted that the initial conditioned reaction (curve *B* is only from a half to a third as intense as the unconditioned lid

[4] This tendency suggests a point of similarity between 'habits' and 'conditioned reflexes' in connection with the 'law of use.' The phenomenon of experimental extinction in conditioned reflexes would seem to contradict the action of this law as applied to habit-formation. This constant upward tendency in the curve of experimental extinction during its early stages, as demonstrated in this investigation, indicates, however, that the first repetitions of the conditioned stimulus tend to re-enforce rather than inhibit the conditioned reaction. To this extent there is agreement between ordinary habit-formation and the conditioned-reflex.

movement (curve *A*). During the first few trials following the initial conditioned wink, practice tends to intensify the reaction. This is shown by the rise in curve *B*. Following this rise there is a steady decrease in intensity of the conditioned reaction until it finally reaches the point of complete experimental extinction. Curve *A* shows that the intensity of the unconditioned reaction itself tends to decline with use.

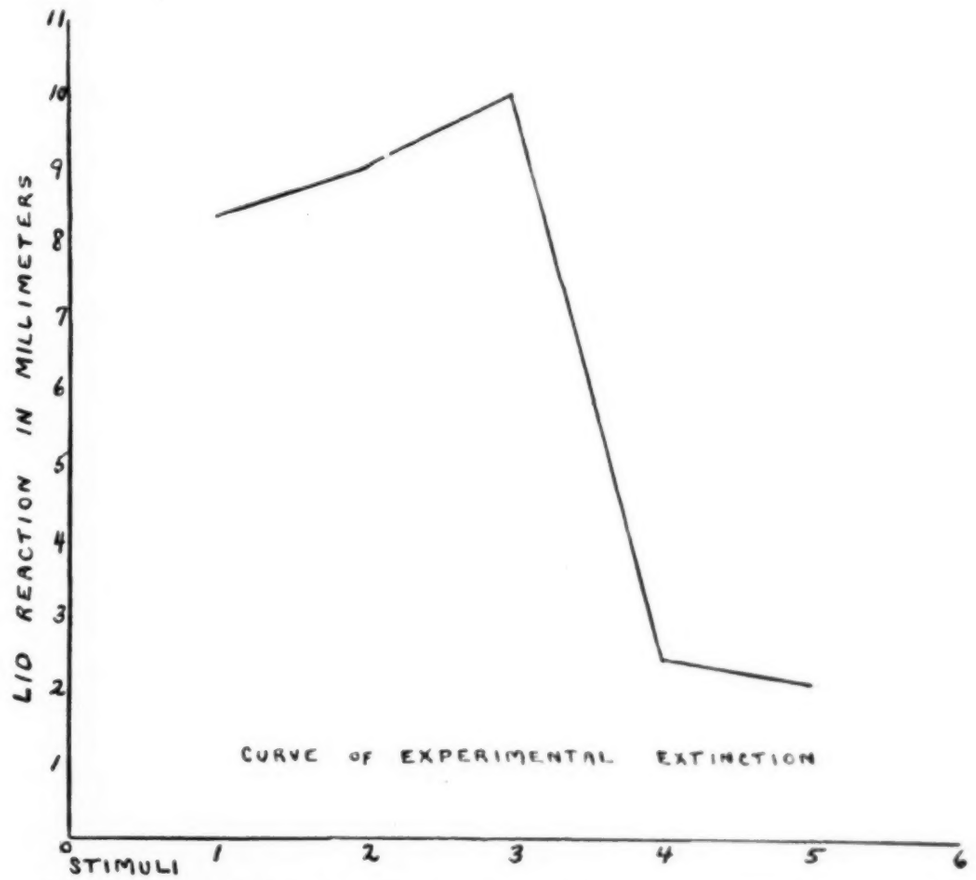

FIG. 6. Mean amplitude of winks recorded for first 5 consecutive conditioned stimulations before experimental extinction reached zero (9 subjs.).

In reviewing Cason's experiment on backward conditioning of the lid reflex (one subject only), it is necessary to consider that the subject sat continuously in the apparatus for five hours. There is, then, first of all, the possibility that fatigue would vitiate any result obtained by the procedure outlined. But even if we disregard the factor of fatigue, we cannot overlook his statement that "the sound stimulus was given a short time after the impulse to wink had been sent

each time, in fact when the natural wink was practically completed." The eyelid reaction has a short but fairly definite refractory phase (12, 45; see also 6, 6). As the sound stimulus was given before the wink was entirely completed, its potency as a conditioning agent may have been thereby precluded. But there is another possible objection to the method employed. The conditioned stimulus (telegraph sounder) and the unconditioned response (wink) were to a large extent under the control of the subject at all times; he could evoke the conditioned stimulus a number of times in rapid succession or defer it for a long interval at will by merely initiating or inhibiting voluntary winks. The set-up was manifestly unfavorable for the establishment of backward conditioning.

The results of the present investigation, illustrated particularly by Subject xx (MB), Fig. 3, bear out the observations of Pavlov (9) and Schlosberg (11, 492) that subjects of the quick, 'nervous' and suggestible type are most easily conditioned. Pavlov found that excitable dogs formed conditioned reflexes most readily and that under conditions causing excitement in the dogs most inhibitions disappeared. Schlosberg found that several human subjects who normally gave no trace of a conditioned knee-jerk gave frequent large reactions upon becoming emotionally disturbed during the course of the experiment.

Supplementary Studies

Patellar Reflex

The apparatus consisted of an extension kymograph, special adjustable chair, cardboard screen, pendulum knee-jerk stimulator, copper net contact fabric for knee, Shipley's knee-jerk recording device,[5] electric key, electric bell, electro-magnetic time marker, and two electro-magnetic stimulus markers.

The subject placed himself comfortably in the chair, and the cord connecting with the knee-jerk recorder was attached to the heel of his left shoe. The screen was put in place in front of the subject, and the stimulator was placed in its sockets and adjusted so that the hammer hit the patellar

[5] Designed and constructed by Mr. Walter Shipley for use in the Laboratory of Experimental Psychology, University of Wisconsin.

tendon squarely below the knee cap. The copper net was then fastened over this spot so that when the hammer touched the knee, the contact incidentally made with the copper net actuated one of the signal-markers, recording on the smoked paper.

The bell was sounded alone three times at the beginning of the experiment to make sure that the knee did not jerk to this sound at the outset. The experimenter next swung the knee-jerk stimulator pendulum backward to about a 45-degree position and released it. When the knee-jerk registered on the smoked paper, the experimenter rang the electric bell by depressing a key. The bell was thus the conditioned stimulus and was given *after* the blow by the stimulator, which constituted the unconditioned stimulus. This combination was repeated at about 10-sec intervals for a number of times, and then the bell was sounded alone (see Fig. 7).

Five subjects were used in this study. Backward conditioning was secured in two cases. Subj. BS (Fig. 7) reacted after 56 re-enforcements with the stimulator. The backward conditioned reflex suffered experimental extinction after two reactions, due probably to the small number of re-enforcements during the training period. Subj. RA reacted to the bell alone after 83 re-enforcements. Conditioning was unstable in this case also, experimental extinction reaching zero after two reactions. Subj. RK, IR, and BH failed to show evidence of backward conditioning of the patellar reflex after 68, 94, and 80 re-enforcements respectively.

Sherrington (12, 45) and Dodge (6, 6) found that the refractory phase of the eye-lid reaction is somewhat less than 500 sigma in most cases, while the refractory phase of the knee-jerk is considerably longer. It would seem, therefore, that there may be a connection between the length of the refractory phase and the number of re-enforcements necessary to establish backward conditioning in any particular reflex. The evidence suggests that a backward conditioning of the patellar reflex can be secured only after a period of re-enforcement relatively much longer than is necessary in case of the lid-reflex.

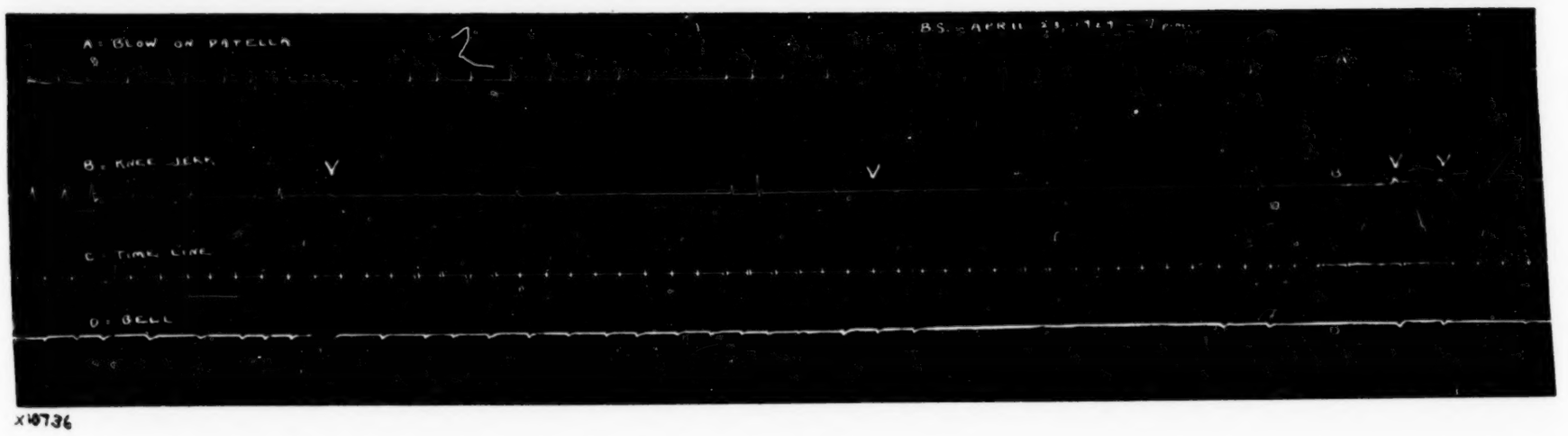

FIG. 7. Backward conditioning of the patellar reflex. White *V* = Conditioned responses to sound of bell. The unphotographed portion of this record contained 48 re-enforcements. The first conditioned response shown in this figure occurred after 56 re-enforcements.

Galvanic Skin Reflex

The apparatus included a kymograph; wall galvanometer, translucent screen, box with slit projecting beam of light upon galvanometric mirror, apparatus for recording movements of the galvanometer beam, chair with an arm containing two finger electrodes; bubbling water stimulator including small water vat, rubber-tube connections with mouthpiece and tambour for recording the stimulation; apparatus for delivering electric shock including switch, rheostat for regulating intensity of shock; wrist electrode with connected batteries and magnetic signal marker; magnetic time marker connected with laboratory master-clock.

The recording device consisted of a square sliding tube with a pointer on a rod at the back of the translucent screen. With this pointer the experimenter was able to follow exactly the movements of the beam of light. As the pointer was moved, a thread attached to it extended to, and moved correspondingly, the larger of two light flanged aluminum wheels. From the smaller of the two wheels a second thread extended to a vertical tracing device for recording on a reduced scale the movements of the galvanometer.

In the form of galvanometer used in this experiment, the current involved in the reaction came directly and entirely from the body of the subject. The shock served as the unconditioned stimulus and was followed after one second by the sound of bubbling water, the conditioning stimulus.

No manifestations of backward conditioning of the galvanic skin reaction were secured in the five subjects tested. The latent period of the galvanic skin reflex is approximately two seconds. As the conditioning stimulus, bubbling water, was introduced one second after the shock, it occurred in each case during this latent period and before the reflex had begun to manifest itself. The training periods ranged between 25 and 50 re-enforcements with shock-stimulus followed by the sound of bubbling water.

Summary and Conclusions

1. Conditioned lid-reactions to a previously neutral sound stimulus were formed in the backward direction, *i.e.* by introducing the conditioning stimulus *after* the unconditioned stimulus.

2. These conditioned lid-reactions were secured in 18 out of 20 subjects (90%) of the Experimental Group.

3. The records of the 10 subjects of the Control Group indicated that the conditioning was backward within each combination and not forward from one combination to the next.

4. The conditioned winks varied considerably in amplitude and frequency of occurrence.

5. Backward conditioning of the lid-reflex was established most readily when the conditioning stimulus followed the unconditioned stimulus at an interval of ½ to 1 second; but it also occurred in some subjects when the interval was extended to 2 seconds.

6. The stability of the conditioned reactions varied markedly with the individuals. In three cases experimental extinction was not complete after sixteen repetitions of the conditioned stimulus.

7. Quick, 'nervous,' suggestible individuals appeared to acquire conditioned lid-reactions very rapidly.

8. Backward conditioning of the patellar reflex was secured in 2 out of 5 subjects.

9. Backward conditioning of the galvanic skin reflex was not established in tests made on 5 subjects.

10. The curve of 'experimental extinction' of the backward conditioned lid-reflex showed a constant tendency toward a sharp rise during the first few applications of the conditioned stimulus, followed by a more or less rapid falling off toward the point of extinction or zero.

11. In most subjects tested the amplitude of the conditioned response was only from one-third to one-half that of the unconditioned response.

12. There was a gradual falling off of the amplitude (diminution in intensity) of the unconditioned response with use.

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OCULAR PURSUIT OF OBJECTS WHICH TEMPORARILY DISAPPEAR

BY ROLAND C. TRAVIS

Western Reserve University

AND

RAYMOND DODGE

Institute of Psychology, Yale University

Introduction

Quantitative knowledge of both normal and abnormal movements of the eyes under a variety of experimental conditions is now available through the use of photographic registration. The five major types of eye-movements, namely, saccadic, pursuit, co-ordinate compensatory, reactive compensatory, convergence and divergence, as described by Dodge (1) in 1903, afford useful points of departure for the systematic study of ocular movements and of their functional significance in adaptive behavior.

We are primarily concerned in this report with a combination of the pursuit and saccadic types of eye-movement, which has been traditionally called 'railroad' or optic nystagmus. It may be described as the conjugate movements of the eyes in maintaining fixation of moving visual objects and is composed of alternate slow and quick phases in opposite directions. Each slow phase is the pursuit of a moving object; each quick phase is the refixation of a new oncoming object. Before defining our specific problem, it may be useful to summarize briefly some of the more important contributions to the objective study of eye-movements of this nature.

Dodge (1) made the first photographic records of eye-movements evoked by a group of moving objects and of those evoked by a moving pendulum (2). The latter type of visual stimulus was subsequently utilized by Diefendorf and Dodge (3) in their study of the ocular reactions of the insane, and more recently by Dodge, Travis and Fox (4) in a systematic investigation of the limits of the adaptive behavior of the eyes in pursuit of an object oscillating in harmonic motion.

The similarity of nystagmus elicited by a succession of moving objects and reflex

nystagmus by labyrinthine excitation has been known for a long time. The latter dies out before the former in the human subject as shown by the photographic records of Dodge (5). The variability of labyrinthine nystagmus has been confirmed and extended by Lorente de Nó (10) in his very thorough and extensive report on the relations of reflexes of rotation to the eye-muscles in the rabbit. Dodge found (6) that photographic records of ocular nystagmus, whether evoked by a succession of moving objects or by rotating the head with the eyes closed, were almost identical. The essential differences seemed to be the manner of excitation and the latency and persistence of the nystagmus. The slow phase of vestibular nystagmus arises from stimulation of the semicircular canals and involves only subcortical pathways, beginning after a latency of the order of .06 sec. The other type arises from stimulation of the retina by a moving object and involves, in addition, higher cerebral pathways, beginning after a latency of the order of 0.20 sec.

Fox and Holmes (7) in their report of the presence and absence of optic nystagmus in pathological patients have shown the value of eye-movements in the localization of cerebral lesions. More recently, Dodge and Fox (8) reported a case with central scotoma in which optic nystagmus was evoked by peripheral retinal stimulation when central vision was excluded. The angular velocity of the slow phase is consequently not determined solely by the impulse to keep the image of the moving object of interest on the fovea, but rather to hold it motionless on a retinal area of relatively clear vision. The same authors studied the variations in nystagmographic records of eye-movements of patients suffering from brain tumors and involvements of the visual and oculomotor pathways (9).

Travis (11), in studying the reciprocal effects of simultaneous initiation of ocular pursuit and the vestibular reflex, obtained eye-movement records showing a 50 percent increase in ocular pursuit latency when vestibular stimulation tended to turn the eyes in a direction opposite to that of pursuit. A 50 percent decrease in ocular pursuit latency was obtained when the two were in harmony.

Essentials of an Instrument for Eliciting and Recording the Ocular Pursuit of Objects Moving at Approximately Constant Velocity

The objects of ocular pursuit in our experiments were narrow wooden bars projecting from the edge of a large wooden pulley which could be rotated by an electric motor in either direction. On rotation of the pulley the extreme ends of the bars crossed the open distal end of a dark tube which was 75 cm long and about 15 cm square in cross-section. The bars were 40 cm long of natural-wood color and when in motion traversed a stationary dark background with which they contrasted. The open distal end of the tube subtended 10 degrees of the visual field while the bars were 15 deg apart, thus only one bar was ever in the visual field at any time.

Opaque obstructions 2 deg, 4 deg, and 6 deg wide were cut from black cardboard and were inserted in tracks at the distal end of the tube. Rectangular windows 2 deg wide extended on either side of each obstruction and permitted corresponding vision of the moving bars and consequent ocular pursuit. Notwithstanding the various sizes of obstructions the window-openings were kept constant in order to standardize the areas of stimulation.

The proximal end of the tube was about 5 cm from the observer's face. A small paper tube (10 cm long and 3 cm in diameter), which lightly touched the skin around the eye, projected into the large dark tube and was held in place by the frame of the mirror recorder. It served to eliminate peripheral stimuli. The observer looked at the bars through the small paper tube with the right eye while the conjugate movements of the mechanically closed left eye were recorded by the Dodge mirror-recorder (12). The beam of light reflected from the mirror fell on vertically moving sensitive paper through a horizontal slit in a recording camera which was mounted on the side of the dark tube. The camera was 60 cm from the recording eye. This gave satisfactory magnification of the eye-movement records. Fig. 4 (below) reproduces a sample record.

A wooden pulley with pins extending from the rim was mounted in front and just above the camera slit, so that the pins cast a shadow across the slit when the latter was illuminated by the reflection of the recording light from a plane mirror. A belt connected the pulley carrying the pursuit bars with the recording pulley; thus an exact record of the onset, stop, and velocity of the stimulus bars accompanied the records of eye-movements. The slopes of the bars and the ocular pursuit records are determined by different physical constants which prevent their direct comparison. The observer's head was stabilized by rests and a mouth-bit.

A record of the shadows cast by the pins of the recording wheel show as white diagonal lines across Fig. 4, beginning at *B*. Fig. 4 also shows dark records of the eye-movements pursuing 5 successive bars as each bar appears in the first 2-degree-window, disappears behind the 4-degree-obstruction and reappears in the second 2-degree-window. The doubling of the dark lines (*C*) which are records of eye-movements is an accident of the position of the recording lamp filament. *A* is a record of the head-movements and *D* is the time-line recording 1/10 sec. The bars were moving 8 deg per sec in this record.

The first bar apparently came out from behind the central obstruction approximately at *2*, the eye made a rapid refixation movement across the 4-degree-obstruction and pursued the bar for a short distance before returning with a rapid refixation movement in the opposite direction. At the edge of the first window it waited for the next bar which came into view approximately at *3*. This bar was successfully pursued completely across the first 2-degree-window, the 4-degree-obstruction, and the second 2-degree-window. The eye returned with a saccadic movement and at *5* the third bar came into view. This bar was pursued part way across the obstruction when the eye remained fixed until the bar appeared at the edge of the second 2-degree-window. Then a saccadic corrective movement for refixation of the moving bar occurred and the bar was pursued until it disappeared at *6*. The return movement was broken at the edge of the obstruction. Approximately at *7* the fourth bar entered the field and was pursued part way across the obstruction; but the eye returned irregularly to the starting point without completing the pursuit. At *9* a fifth bar entered the field and was pursued to the edge of the obstruction. Here the pursuit-glide gave way to a saccadic movement, the eye fixated the edge of the second 2-degree-window where the

moving bar was picked up and pursued irregularly out of view, at *10*. This figure showing the records of ocular pursuit of five successive bars which disappear for a time behind an obstruction illustrates all the major forms of ocular behavior which we found to occur under these conditions.

The Orientation of Our Present Problem

This report is primarily concerned with the exploration of ocular behavior when a moving object of regard disappears for a time behind an obstruction. It is part of a study of normal and abnormal ocular behavior in pursuit of moving objects.

The problem bears on the initiation and control of response, which was reported by Dodge (13) in his study of human variability, and by Travis (11) in his study of the reciprocal effects of ocular pursuit and the vestibular reflex. It concerns neural relationships in voluntary control of adaptive behavior and is of fundamental importance in the description of the interaction of neural centers in the control of the final common path of a given synergic system (13*b*).

Eye-movements play an important rôle in many of our complex mental activities and in adaptive behavior, yet they are peculiarly inaccessible to subjective observation. The only type of eye-movement which is ordinarily initiated voluntarily is the saccadic, or the refixating movement in optic nystagmus, and once begun even this seems to pass from voluntary control. The pursuit-movements which concern us primarily in this report seem particularly dependent on adequate stimuli. When the gaze is once fixed on an object, the ocular muscles through their neural connections automatically determine such movements of the eyes as are necessary to keep the image of the object fixed on the retina. A patient may be unable to look to the left in order to bring an object into view; yet if he is told to fixate some object held before him, and his head is passively turned to the right, he may still be able to maintain approximate fixation of the object by a reflex compensatory movement of the eyes (14). The patient unable to make eye-movements voluntarily is able to do so reflexly in order to hold the object in view. Such facts seem to indicate two different neural mechanisms for the control of slow drifts and saccadic eye-movements.

Modification of eye-movement patterns by practice is of primary importance in further differentiating neural origins and in establishing the functional significance of types of ocular behavior in the adaptation of the organism. Whether ocular pursuit of a moving object which disappears for a time behind an obstruction could be improved through practice was a part of our experimental problem.

Relation between Adequacy of Ocular Pursuit and the Velocity of the Object of Regard Moving in the Horizontal Plane

The two solid lines *A* and *B* in Fig. 1 show the relation between adequacy of ocular pursuit and the velocity of the moving object of regard for the 10-degree and 5-degree

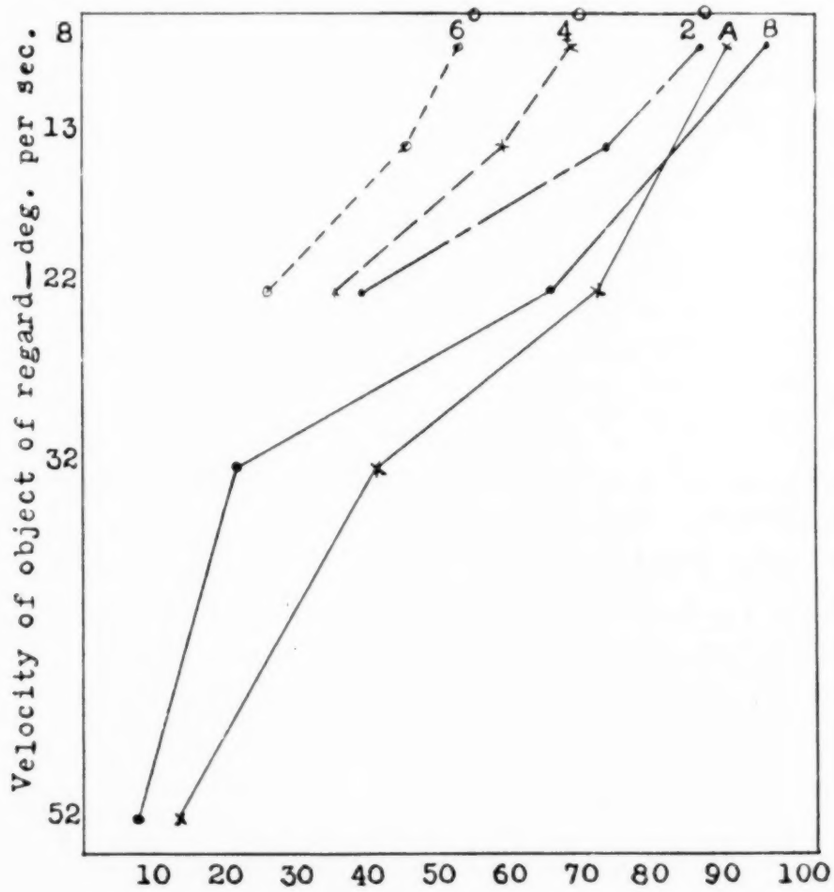

Fig. 1. Relation between adequacy of ocular pursuit across obstructions (3 broken lines), unobstructed ocular pursuit (2 solid lines), and velocity of the object of regard. Each point equals average of 200 responses. Adequacy (percentages) of ocular pursuit.

pursuit-fields respectively. Adequacy of pursuit is given in percentages of the objective pursuit-field actually traversed by ocular pursuit. The velocity of the moving object is given in degrees per second. The slopes of the lines show a decrease in percentage of adequate ocular pursuit roughly proportional to the velocity of the bars from 8 to 52 deg per sec. At 8 deg per sec the pursuit is nearly adequate, whereas at 52 deg per sec the adequacy has decreased to 10 percent, apparently approaching the physiological limit of the eye to follow moving objects. This decrease in adequacy may be somewhat exaggerated for small angles in consequence of our specific experimental situation. The observer was instructed to watch each bar as long as it was in the field of view. When one bar disappeared the eye made a saccadic jump to the edge of the field at which the oncoming bar was expected. Thus two voluntary processes seem to be involved in the artificial nystagmus of these experiments, one was the pursuit of each bar as long as it was visible, the other was a return quick enough to pick up the oncoming bar as it appeared. As the velocity of the moving bars increased, the time-interval between the appearance of successive bars decreased. The experimental situation at high velocities made increased demands on the speed of ocular adjustment to complete the rhythm of pursuit and refixation. Just how far the consequent decreased adequacy of pursuit was conditioned by ocular latency seemed impossible to determine from our records. Therefore only the main changes in adequacy of pursuit are considered in the report. There is evidence, however, that our somewhat unusual conditions do not materially alter the picture obtained when the subject merely attempts to fixate a continuously moving field.

Ocular pursuit for the 10-deg pursuit-field is slightly better than for the 5-deg field, at the higher velocities. The greatest difference between the two occurs at 32 deg per sec. At 8 deg per sec, the 5-deg field is somewhat more favorable for pursuit than the 10-deg field. Each point in these two curves represents the average percentage of adequacy of ocular pursuit of 200 stimulus bars, including both clockwise and counterclockwise directions. The data obtained from the 5- and

10-deg fields when compared with adequacy of ocular pursuit under somewhat different conditions with a 30-deg field show almost rectilinear direct proportionality with the extent of the field at velocities of 52, 32, and 22 deg per sec. Adequacy in ocular pursuit for the 30-degree field is about 30–40 percent higher than for the 10-deg field, and about 6 percent higher for the 10-deg field than for the 5-deg field, at the same velocities of the moving object of regard.

Relation between the Adequacy of Ocular Pursuit and the Velocity of the Moving Object of Regard when the Object Disappears for a Time behind an Obstruction

The three broken lines in Fig. 1 show the relation between adequacy of ocular pursuit and the velocity of the moving object when the obstructions were 2 deg, 4 deg, and 6 deg, respectively. The average percentages for both clockwise and counter-clockwise directions are plotted against the velocity of the moving object of regard in degrees per second. These obstructions were oriented centrally in the visual field with 2 degrees of clear vision of the moving bars on either side, irrespective of the length of the obstruction.

An analogous decrease in the adequacy of ocular pursuit as the velocity of the bars increases is manifest in ocular pursuit across obstructions as in unobstructed pursuit, but at a different level. The obstructions offer definite barriers to ocular pursuit; the longer the obstruction the greater the barrier. The latter relationship is shown more clearly in Fig. 2. The effect of the obstructions is notably greater for the slower velocities of the bars than for the higher velocities.

These data seem to indicate that the ocular pursuit of a moving object is a kind of short-lived motor habit dependent upon a very specific type of stimulus—the image of a moving object falling on the retina. They also indicate that the pursuit-reaction continues for a short time after the stimulus has disappeared. The variability in this respect is very great as shown by the 5 successive responses in Fig. 4. In a small

percentage of cases for one subject ocular pursuit continued with the initial velocity, after the disappearance of the stimulus, for 0.8 sec, and for 0.5 sec for the other 3 subjects. Even when the bar disappeared for only 0.25 sec, at a velocity which otherwise elicits quite adequate pursuit, the average

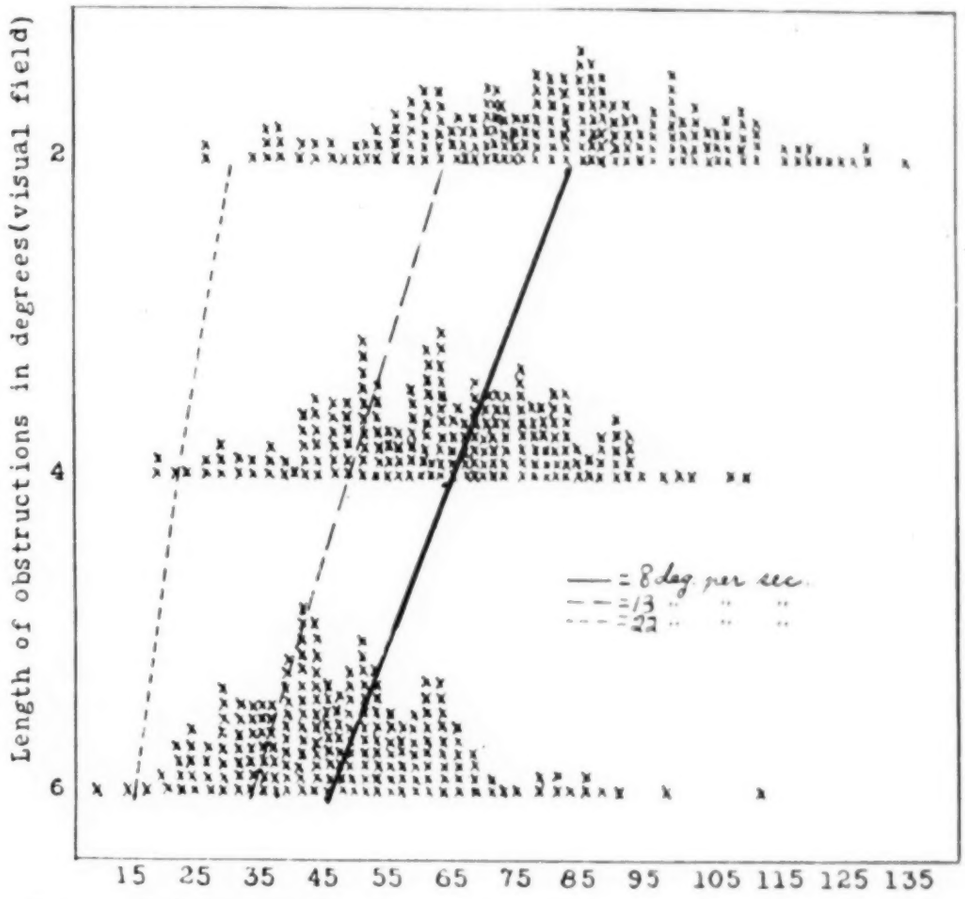

FIG. 2. Relation between adequacy (in percentages) of ocular pursuit across an obstruction, the length of the obstruction, and velocity of the object of regard. Clockwise pursuit. Distribution for 8 deg. per sec velocity only. Percentage—ocular pursuit.

percentage of adequate pursuits for all four subjects is twenty-one for clockwise and forty-six for counter-clockwise movement, as is shown in Table I.

These facts present evidence that in response to short exposure of moving objects slow movements of the eyes longer than one second are probably impossible without adequate moving stimuli. At longer time-intervals without adequate stimuli, pursuit gives place to movements of the rapid or

saccadic type. Even at 0.5 sec without adequate stimuli ocular pursuit is seriously disturbed in most of our subjects.

Relation between the Adequacy of Ocular Pursuit of Moving Objects which Disappear for a Time Behind an Obstruction and the Length of the Obstruction

The distributions in Fig. 2, represent the percentages of adequacy of ocular pursuit for one velocity (8 deg per sec) of the pursuit-bars for the 2-deg-, 4-deg-, and 6-deg-obstructions. The heavy diagonal line connects the means of the distributions. Corresponding means of the distributions for the velocities of 13 deg per sec (long dashes) and 22 deg per sec (short dashes) are indicated by the broken lines. The distributions for these two speeds are not reproduced; but they have a spread and grouping similar to those of the 8-degrees-per-second velocity. At all three velocities, as the obstructions became longer the adequacy of pursuit decreased proportionately, as shown by the several means. The greatest decrease occurs in case of the 8-degree-per-second velocity.

The spread of the percentages of ocular pursuit is indicative of the great variability of responses obtained in attempting to pursue successive bars as they disappear for a time behind an obstruction. These distributions also indicate how inadequately averages represent the various facts of eye-movement. Only one adequate pursuit is shown in Fig. 4, viz., between points *3* and *4*. Table I shows (in percentages) the frequency of the entirely adequate pursuits across the several obstructions. The greatest number of adequate pursuits occur with the 2-degree-obstruction.

Table I

Frequency of adequate pursuits across obstructions

	6 deg-obstr'n clockw. speeds			6 deg-obstr'n c-clockw. speeds			4 deg-obstr'n clockw. speeds			4 deg-obstr'n c-clockw. speeds			2 deg-obstr'n clockw. speeds			2 deg-obstr'n c-clockw. speeds		
Subj.	8	13	22	8	13	22	8	13	22	8	13	22	8	13	22	8	13	22
C	0	6	0	0	0	0	0	14	1	0	2	0	47	38	0	39	31	0
T	0	0	0	0	0	0	4	4	0	11	10	0	32	35	14	38	29	7
MH	0	0	2	0	8	24	0	0	0	11	32	39	0	9	8	60	54	43
EH	1	0	0	9	0	0	6	0	0	12	9	1	6	0	1	47	14	30
Ave	0	1	0	2	2	6	2	4	0	9	13	10	21	20	6	6	32	20

Comparison of Clockwise and Counter-Clockwise Ocular Pursuit

Table II gives the average percentage of adequacy of ocular pursuit, without the obstructions, for each direction and for each subject with 10 degrees and 5 degrees of visual field.

Table II

Percentages of adequacy of simple pursuit, clockwise and counter-clockwise

Subj.	10 degree-field clockwise	10 degree-field c-clockw.	10 degree-field diff'ce	5 degree-field clockwise	5 degree-field c-clockw.	5 degree-field diff'ce
C.	54	46	−8	70	51	−19
T.	51	58	+7	47	50	+ 3
MH.	53	70	+17	36	63	+27
EH.	48	59	+11	35	37	+ 2

The differences in favor of the counter-clockwise pursuit are preceded by a plus sign. Individual differences in adequacy of ocular pursuit between the two directions are quite marked. Three of the four observers show better counter-clockwise pursuit than clockwise, whereas the reverse is true for Subj. C, who is left-handed.

The differences between the two directions are still more striking in ocular pursuit of a moving object which disappears for a time behind an obstruction, as shown in Table III. Here the bias in favor of the counter-clockwise direction increases for all subjects.

Table III

Percentages of adequacy of pursuit across obstructions, clockwise and counter-clockwise

Subj.	6 deg-obstr'n clockw.	6 deg-obstr'n c-clockw.	6 deg-obstr'n diff'ce	4 deg-obstr'n clockw.	4 deg-obstr'n c-clockw.	4 deg-obstr'n diff'ce	2 deg-obstr'n clockw.	2 deg-obstr'n c-clockw.	2 deg-obstr'n diff'ce
C.	39	45	+ 6	55	54	− 1	72	74	+ 2
T.	31	45	+14	53	62	+ 9	62	71	+ 9
MH.	28	68	+40	38	85	+47	63	108	+45
EH.	29	50	+21	33	59	+26	40	64	+24

The meaning of these differences in ocular pursuit between the two directions in the two situations is not clear. In a still unpublished report, Dodge, Travis and Fox (4) found no observable difference between clockwise and counter-clockwise ocular pursuit of an object moving in harmonic motion. Apparently the difference is not primarily in the slow pursuit

phase of optic nystagmus; but, on the basis of all our evidence, it must be attributed in greater part at least to the readiness of the saccadic phase. That there is a probable difference between the saccadic eye-movements in the two directions was pointed out by Dodge (15) in the earliest measurements of the angular velocity of eye-movements from photographic records. Exact records under still more refined photographic methods must be obtained definitely to settle this point.

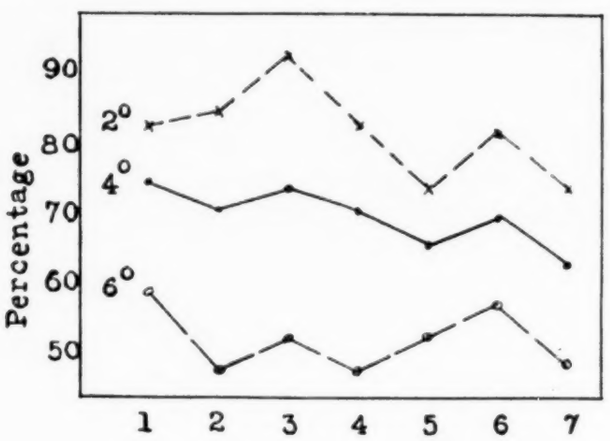

FIG. 3. Variability from day to day in adequacy of ocular pursuit across obstructions, 2 deg, 4 deg, and 6 deg. (Days.)

In their comparative studies of normal-speaking individuals and stutterers, Orton and Travis (16) found from photographic records differences in the onset of action-currents between the right and left sides of the body in the majority of cases. These studies bear on the problem of hemispheric dominance in the initiation of voluntary response, and suggest a possible interpretation of the differences between clockwise and counter-clockwise pursuit.

MODIFIABILITY OF OCULAR PURSUIT ACROSS OBSTRUCTIONS

Fig. 3 shows the daily variation in the adequacy of ocular pursuit of moving bars disappearing for a time behind obstructions of 2 deg, 4 deg, and 6 deg. On the whole there appears to be a slight decrease in adequacy as the experiment continues from the first to the seventh day. Notwithstanding the fact that each subject was urged to try to improve on his previous day's performance, the average of the first three days

compared with the average of the last three days showed an average decrease of adequacy of 6 percent. This corresponds with the decrement found by Dodge in various reflexes (13, *a*).

In consideration of the probable neural origins of the pursuit phase, improvement through practice could hardly be expected; but too little is known of the neural relationships involved in the whole process of ocular movement to predict

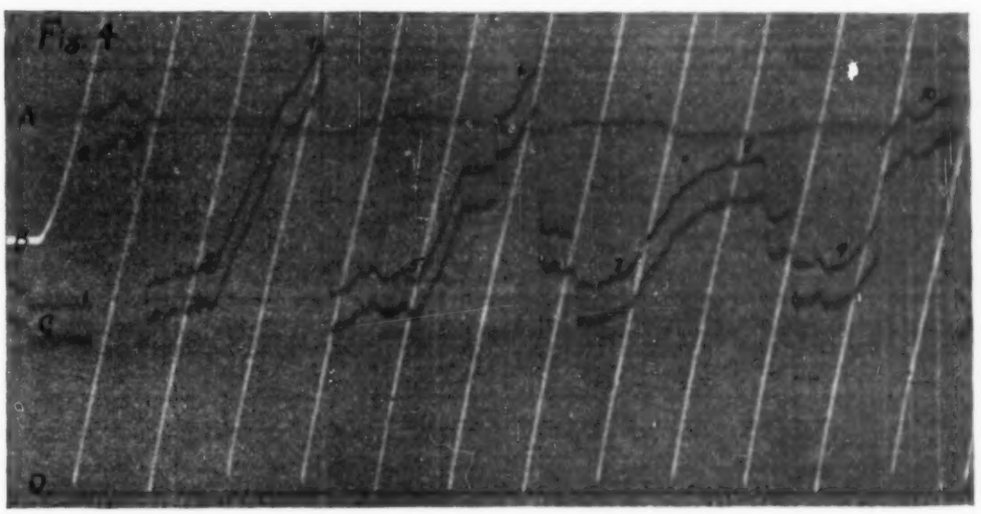

FIG. 4. Reproduction of photographic record of ocular pursuit of a succession of moving bars which disappear for a time behind a 4-degree-obstruction. Subj. EH. *A* is head line; *B* records the onset of motion and speed of the bars; *C* is record of eye-movements and *D* shows time in 1/10 sec.

what effect practice might have on particular ocular patterns, especially those including short-lived motor habits.

Bott, Brown and Cohen (17) reported a very regular and decided improvement in a binocular motor pattern involving the depression or elevation of one eye to maintain binocular fixation; but this pattern probably involves neural relationships entirely different from those of ocular pursuit.

The following comments on the experiments by Mr. E. R. Hilgard (Subj. EH) seem instructive:

"The observed field consisted of two openings, left and right, with a dark obstruction between them. The stick moved across the windows and behind the obstruction. It is to this setting that the following introspective comments apply.

(1) The 'natural' method into which I seemed to fall with lessened attention was clearly to look from one window to the other, disregarding anything happening behind the central block. It required felt effort to follow the instructions to pursue across the obstruction at the same rate as across the windows.

(2) Pursuing across the obstruction suggested the using of subjective aids. Success could be roughly judged by the reaching of the edge of the obstruction focally just as the stick appeared.

(*a*) The first aid tried was imagining the stick to move behind the obstruction, with the attempt to follow it as though it were there perceptually. This was very strained and not very successful, and was never used after the first day.

The only exception (if it is an exception) occurred when the motion was rapid enough and the central block of small enough size so that the motion appeared to be continuous. I have thought of this as a Phi-movement. There was then no strain of attention in keeping the set for continuous pursuit.

(*b*) The second aid, naturally fallen into, and subsequently carefully employed, may be described as a dynamic relation between the moving eye and the moving stick. The eye seemed to sweep the stick across the field. The sweeping motion allowed no breaks at the edges of the obstruction, and the motion was therefore according to instructions. As another analogy expressing the dynamic relation between eye and stick, the relation was thought of as that between a moving magnet and an armature behind a cardboard screen. The interpolation of the screen does not interrupt the smooth motion of the armature.

(3) The necessity for refixation introduced variation in the attentive set from time to time within the same series, and particularly from one size of obstruction to another. Thus, increasing care to fixate the edges of the openings (to make the full excursion) tended to interrupt the rhythm of the sweeping movement by slowing the start. It was difficult to balance the two acts, both of which strained attention: (1) to pick up the speed quickly so that pursuit across the obstruction would be appropriate, and (2) to fixate the very beginning and to pursue to the very end of the movement. It was more natural to fixate ends and to jump, or to keep up a swinging sweeping movement.

The separation of the windows had two effects. (1) The lesser arc of movement seemed somewhat easier. (2) There was with the closer windows more time for refixation, since the second stick appeared at a longer interval after the disappearance of the first than in the case of windows wider apart, when the second stick appeared almost immediately after the disappearance of the first.

(4) Certain accompanying movements and sensory experiences should be mentioned.

(*a*) Something frequently happened at the edges of the obstruction. The sudden change of illumination as the bright stick appeared and disappeared may have had something to do with it. The set to pursue across the dark block may have been responsible. This 'something' was an experience of change which I am not prepared to describe sensorially. There was often a squint, however, which was clearly felt.

(*b*) The sweeping movement which I have described as a dynamic relation between the eye and the moving stick was often accompanied by motion of extraneous muscle groups, as about the eye, in squinting, or in the jaws, or at the back of the head where changing pressure against the rest suggested incipient head movements.

(5) I should perhaps give my general reaction to the apparatus. It was altogether comfortable, and sessions were short, so that there was no sensible fatigue.

The spectacle-like support and the recorder attached to the left eye were comfortable and forgotten during the course of the experiment. The time-marker was not heard without special attention, and the noise of rotation was never thought of unless mentioned, and it never seemed a distraction. I think of no extraneous factors in the situation to modify the responses I was making."

Summary

1. This paper reports a continuation of the exploration of the slow or pursuit phase of optic nystagmus. In particular, we measured the amplitude of the pursuit-glide as an indication of its adequacy under various experimental conditions.

2. Within the limits of our experimental situations the adequacy of horizontal ocular pursuit varied inversely with the velocity of the moving objects of regard. The higher the velocity of the moving object the less the adequacy.

3. The adequacy of ocular pursuit of moving bars which disappear for a time behind an obstruction varied inversely with the velocity of the bars and with the length of the obstruction.

4. Simple pursuing or gliding movements of the eyes disappeared in all our subjects when the object was 0.8 second or longer in passing behind the obstruction.

5. There was marked variability for each day and from day to day in the adequacy of ocular pursuit of disappearing bars. Averages of the first three days when compared with the averages of the last three days showed a decrease in adequacy of the pursuit of about 6 percent.

6. In view of our data, ocular pursuit of moving objects may be regarded as a kind of short-lived habit which develops in each instance of pursuit and shows marked variation from experiment to experiment.

7. Ocular pursuit was more nearly adequate for three of the four subjects for counter-clockwise movement of the bars with and without the obstructions. The exceptional subject was left-handed.

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